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LIMITATIONS OF PLANNING IN SCIENTIFIC RESEARCH

THE best form for the central organisation of scientific effort in Great Britain is so complex a problem that it may be wise not to determine it finally with undue haste. We should have regard to the orderly evolution that marks the advance of science, and be on guard against rash generalizations and unsound analogies just as much as against the tendency to excessive planning at the centre. In this connexion, a review by Dr. F. B. Jewett of the future of scientific research in the post-war world (*Amer. Sci.*, **34**, 439 ; July, 1946) should be studied. Dr. Jewett does much more than sound a note of caution as to whether there is not a limit to the speed at which society can wisely assimilate radically new scientific developments *en masse* into the social structure. He is concerned both with the effect on the balance of society, and, what is much more commonly overlooked, with its bearing on the attitude of the man of science himself towards his work as well as towards the society of which he is a part. Sir Henry Dale, in his presidential address at the Dundee meeting of the British Association, was obviously concerned with this latter aspect, though unlike Dr. Jewett he made no reference to the importance of further stimulating research into other fields of knowledge than the physical or biological sciences. In this connexion the forthcoming report of the National Committee on the Social Sciences set up by the Association of Scientific Workers (see p. 68) will be of interest.

The broad picture of the present conditions for scientific research drawn by Dr. Jewett is generally similar to that outlined by Sir Henry Dale, but he has filled in more of the detail. He reminds us that in fundamental and applied science the corps of highly trained research men is substantially the same numerically as at the beginning of the War ; but its average age is higher by four or five years and its creative potentiality has probably decreased. The next lower age-group of fully trained men is almost entirely lacking, and although partially trained men from the Services or war-work may ultimately fill the gap after completing their training or on the basis of experience, many will not be productive for some time. There is also a dearth of those who would normally be candidates for post-graduate training.

The contour and extent of fundamental knowledge do not differ greatly from those of four or five years ago ; but in the field of applied science, large extensions have been made in some sectors and none at all in others. A major problem of industrial research is to secure a better balance of technology in relation to social needs. The range, diversity and often the power of research tools have been greatly increased, and in some sectors tools, methods and materials previously rare, expensive or difficult to obtain, have been made comparatively easily available. The interdependence of different sectors of science for their mutual advancement has also been

strikingly demonstrated during the War, just as have the value of new methods of planning and organising research, of team-work and the whole technique we know as operational research.

Only time can rectify the shortage of scientific man-power which Dr. Jewett and his colleagues in the United States emphasize, just as much as is done in Great Britain; but wise planning and the right priorities can speed up the process. As to the subjects of research, Dr. Jewett expects that if science is afforded the freedom it needs, it will for the next few years be mainly concerned with developing the fields on which interest is at present focused. He anticipates, however, a much wider use of, and greater demand for, the mathematician; and that there will be increasing co-operation between mathematicians and physical scientists; and in the field of applied science he anticipates a considerable expansion in the size and number of co-operative laboratories of the type of the Mellon and Battelle Institutes.

Dr. Jewett is concerned, however, that there should be no deterioration in the quality of research; and it should be remembered that this is one of the factors to be considered at every level in production. As Mr. A. C. Bossom observed in the concluding lecture on economic reconstruction delivered at the Royal Society of Arts last May, austerity production does not equip men to make better, or craftsmen's, articles. The manufacturing fame of Britain rests largely on the craftsmanship responsible for the high quality of our goods, and there is no more important aspect of industrial efficiency than this. It is one to which joint production committees and any other means we adopt for securing better industrial co-operation must give close and continuous attention.

It is in ways like this that the collaboration of the social scientist with the worker in the physical or the biological sciences can prove so fruitful; indeed, it should be remembered that the development of scientific management and the improvement of methods of administration do to some extent involve further research into problems of social science, though primarily they involve the wise and thorough application of existing knowledge and of the scientific approach.

It cannot in truth be seriously maintained that the question of the best deployment of the resources of scientific man-power in Great Britain is a matter to be resolved entirely by the technique described as 'operational research'. No doubt both the Advisory Council on Scientific Policy, the Defence Research Policy Committee and the Committee on Industrial Productivity between them can do something to indicate the most profitable distribution, and even enlist the co-operation of industry in the attack on research problems. But it is difficult to visualize them as instigating or directing a redistribution of scientific effort on the scale suggested by the Association of Scientific Workers, or to believe that the change-over could be effected rapidly without serious loss of efficiency or waste of man-power and resources. Prof. D. H. Robertson has suggested that the direction or mobility of labour will be most satisfactorily secured for all concerned by the operation of the old price system in a free economy. The reception of the Control of Engagement Order indicates that he may be right, and this may be as true at the scientific as at any other level.

The technique of operational research may be used, however, to help in determining the most profitable targets, and in organising the co-operative attack on the selected objectives. It could also foster the conception of the effectiveness and non-effectiveness of industrial research according to the opportunity and help we gave it to fructify which Sir Clifford Paterson commended for the consideration of the Royal Society as a means of ensuring economy in scientific man-power. Presumably the Committee of the Fabian Society had such ideas in mind when, in its recent report on the Civil Service, it suggested that the Civil departments, and particularly the Board of Trade, might use such a technique. 'Operational research' has undoubtedly assisted the administrator and the numerous types of expert to combine in the attack on a single problem, and there are few problems in our industrial and economic, not to mention our political, life which do not involve such co-operation. It may well be that some such technique could make an outstanding contribution in the field of industrial psychology, or industrial anthropology, which has such a vital bearing on industrial efficiency and morale to-day. The investigation of such matters and into problems of industrial administration which Prof. P. Sargant Florence suggested in a thoughtful article, "Incentives and Government Policy", in the *Political Quarterly* is one field in which the technique should be indispensable.

The importance of this needs no further emphasis than Field Marshal Montgomery and General Slim have recently given it, for morale in industry means as much to success as in military operations, and can be studied as scientifically as was the morale of the fighting man in time of war. Nor are the other conditions of success so dissimilar in the two spheres: simplification of the problem, the choice of subordinates and reliance upon them, courage and determination to achieve a clearly visualized objective —these have just as much to do with the attainment of scientific or industrial objectives, and no mere system of organisation can be a substitute for the wisdom and moral qualities they imply.

We already possess in Britain in large measure the administrative organisation that is required. What has to be done is to ensure that it is used effectively and wisely, that it is staffed with men competent to take the right decisions and to select the right targets, and to keep a proper balance between long-term and short-term objectives. We need to encourage the wider use of the techniques and methods which progressive firms found fruitful in technical or scientific service to industries using their products long before the term 'operational research' was introduced, while avoiding the fostering of extravagant hopes. We have to secure the co-operation of every scientific worker, in the fields of work in which he is immediately engaged, through his professional associations

making representation as to what is required, and no less on the regional research councils, production committees and other bodies through which scientific and technical opinion can be clearly and effectively expressed on the problems confronting the nation, industry and commerce, and the means by which they can be attacked. There are limits to human judgment and to human powers of organisation; but within those limits it is possible to plan the use of the scientific and material resources of Britain with some approach to relative, if not to absolute, efficiency, and to avert the encroachment of political tyranny on scientific or any other form of human competence. But we must recognize the limits of planning, and keep to a minimum the burden of those central decisions which at present so often tend to clog the machine and to stifle rather than to encourage initiative, enterprise and judgment.

SUPERCONDUCTIVITY

Theorie der Supraleitung

Von M. von Laue. Pp. iii + 124. (Berlin und Göttingen: Springer-Verlag, 1947.) 12 R.marks.

THERE are few theoretical physicists who at one time or another have not tried to develop a theory of superconductivity; but all attempts so far have been unsuccessful. It might have been expected that it would be possible to explain such a spectacular phenomenon in a simple way in terms of the properties of electrons in metals. The conclusion has to be drawn, therefore, that we have not yet learned to derive in a satisfactory way the features of an assembly of electrons from their individual properties. Similar failures in other branches of physics (for example, liquid helium II, or the mechanical properties of solids) indicate that these difficulties are not restricted to electrons but concern many-body problems in general.

In discussions on this subject it is often suggested that a more successful treatment of the many-body problem requires an improvement of the mathematical technique rather than new physical ideas. Yet even an attempt to derive the properties of ideal gases would find the atomic physicist (if assumed to have no knowledge of macro-physics) at a loss without the introduction of new physical concepts. He would probably start with a discussion of the motion of two and then of three weakly interacting particles, and afterwards be led to the conclusion that consideration of more particles is very complicated and unlikely to lead to any simple results. It is only after the introduction of new physical concepts which do not exist in atomic physics—such as pressure and entropy—that other simple laws of physics (the gas laws) can be found. It may well be that further concepts of this kind will be discovered and that they will be essential in solving problems like superconductivity. An important step in this direction is the formulation of an adequate phenomenological theory.

In the case of superconductivity, this step was taken about twelve years ago by the brothers F. and H. London, and M. von Laue has now written an excellent monograph on this theory. He starts with a short discussion of the two basic electromagnetic properties of superconductors, the existence of permanent currents and the magnetic properties, and then proceeds to formulate the fundamental equations of the Maxwell–London theory. Maxwell's equations, as is well known, require a number of supplementary equations specifying the properties of the substances in the electromagnetic behaviour of which one is interested (for example, Ohm's law in the case of non-superconducting metals). London's equations for superconductors consist of a differential correlation between the electromagnetic field vectors and the current density. In conjunction with the Maxwell equations, they give rise to interesting developments which are very thoroughly treated in von Laue's book. The book continues with a discussion of space charges in superconductors and of conservation of energy. There follows a detailed treatment of a number of specific cases such as cylinders and spheres in magnetic fields, the properties of permanent currents (requiring theorems on multiple connected spaces), electric waves and high-frequency resistance, and others. After an outline of the thermodynamics of the transition from the normal to the superconducting state, the book finishes with two sections on the properties of thin layers and of the intermediate state.

One might find the section dealing with the thermodynamics of superconductors comparatively short, but then as von Laue points out in the introduction, his book aims at giving an account of the London theory only. Unfortunately, he does not quote the two original papers by F. and H. London (*Proc. Roy. Soc.*, A, **149**, 71 (1935); *Physica*, **4**, 341 (1935)), but only a later review by F. London, and he attributes the whole theory to the latter author only. The responsibility for the contributions was, in fact, equally shared by the two authors. It is therefore to be hoped that a suitable correction will be made in future editions to eliminate a misleading impression.

The book is written in a very thorough manner; all theoretical results are derived in detail and difficulties are indicated and discussed. Some of these results will no doubt stimulate new experiments. The book will be read with profit by all interested in the theory of superconductivity. H. FRÖHLICH

NATIONAL INTELLIGENCE

The Trend of National Intelligence

By Prof. Godfrey Thomson, with a Symposium by Sir Alexander Carr-Saunders, Sir Cyril Burt, Prof. Lionel Penrose and Prof. Godfrey Thomson. Pp. 35. (London: The Eugenics Society and Hamish Hamilton.) 2*s*. net.

IN the Galton Lecture for 1946, Prof. Godfrey Thomson marshalls the evidence that leads him to the conclusion that there is at the present time in Britain, and has been for at least twenty-five years, a correlation of approximately −0·25 between the intelligence score of a child of about eleven years and the size of the family of which he (or she) is a member. The facts are no longer in dispute; yet it makes a profound difference to the points of policy on which emphasis might be laid, if any attempt were made to reverse the present tendency, whether the interpretation of the facts is that the average level of innate intelligence in the country is declining, or that the facts merely indicate a differential environ-

mental influence on intelligence test scores. To obtain an accurate interpretation would involve, as Carr-Saunders and Burt point out in their contributions to the symposium in 1947 which followed Thomson's Galton Lecture, a team of research geneticists, psychologists, statisticians and sociologists backed by a heavy endowment. Thomson's interpretation, on the basis of his own and other people's carefully prepared investigations, is that hereditary factors are causing a decline in the national intelligence of at least one point in *I.Q.* per generation, and probably more.

Thomson would be the first to admit that his conclusion marks the opening of a new, rather than the closing of an old, chapter of investigation. For if it be true that the average level of national intelligence is declining as a result of hereditary factors, then a number of questions affecting future policy at once spring to one's mind. Is the decline to be attributed to an increase in the absolute number of those with less intelligence than the average, or to a decrease in the absolute number of those with more, or to both? And which of these is the more important or disastrous socially? Is the change one that is affecting all intelligence-levels or only those individuals at either or both extremes of the range? To what extent does a decline in innate intelligence imply a decline also in other desirable characteristics (since many mental qualities are positively correlated)? Is the true position merely that the more intelligent people are having fewer children, or are we justified in making the further deduction that the differential birth-rate is adversely affecting innate intelligence?

Again, one needs to know whether the decline is one that has been continuing for a number of generations or whether it is a modern, and perhaps temporary, phenomenon. In this connexion one would need to know how long differential fertility had been a significant problem. Thomson is rightly concerned by the fact that the educational system of Britain sieves the more intelligent, gives them a longer education and prepares them for a profession in which they will marry later and have fewer children than the less intelligent. Yet if the differential birth-rate should happen to have been a significant factor in the decline of intelligence over the past centuries, then are there any influences to mitigate these effects and to counteract our decline into a nation of defectives? That there may be some factor to be discovered is suggested by Penrose's investigation into the height of Toronto children. A very small but significant inverse correlation between height and sibship provided the basis for the expectation that the mean height would decline at the rate of about a quarter of an inch a generation. Measurements in 1923 and 1939, however, indicated that during this half generation the average height had actually increased by between 1 and 2 in. It may be, as Thomson suggests, that better nutrition or other environmental conditions have temporarily masked a genetic decrease, and that improved educational nutrition might similarly, though only temporarily, mask a decline in inborn intelligence.

If this should be so, it would be arguable that social policy might profitably be directed along these positive lines until the genetic factors underlying the decline can be more clearly formulated. For if it be true that nutrition can affect to such a startling degree something with so considerable a hereditary basis as height, then how much more might it not be worth while to investigate the effect of educational nutrition on intelligence test scores?

It is now time to remind ourselves that all the predictions about the future trend of innate intelligence are based on intelligence test 'scores', and that a great many more investigations need to be undertaken into the extent to which such scores are affected by environmental handicaps or advantages. To what extent is the fact that the lower *I.Q.*s are found in the larger families associated with the fact that the larger families are also to be found among those with fewer social advantages? Thomson's important investigation into a socially very homogeneous group was an attempt to answer this question; yet even here the negative correlation between intelligence score and family-size might be reasonably interpreted in terms of the more extreme poverty of those with the larger families in a group that was altogether very poor (coal miners in 1926).

The fact, too, that different average *I.Q.*s are often obtained from different groups, for example, rural and urban, northern and southern, etc., raises questions about the relative fairness of the items included in a single test given to both groups: it also raises questions about the original standardization of the test, for example, whether the population on whom the test was originally standardized corresponds fairly with the population that has been selected for the purpose of a special investigation. Again, further investigation needs to be made into the extent to which individuals with very different types of mind can obtain the same score on an intelligence test; and of the effect on their relative scores of different methods of grading the marks on a test. As a corollary to this question, more needs to be known about the extent to which individuals with the same type of mind obtain different intelligence scores. Even identical twins rarely obtain identical scores; what, then, is the reason for, and what the significance of, their differences? Are they an indication of the influence of pre-natal or post-natal environmental differences, or that the measuring rod itself, the test, is unreliable? If the latter, is this unreliability constant for all groups of whatever social class, or does it vary from one class to another?

I have been able to do little more than touch on some of the implications of Thomson's Galton Lecture. How far any answer to these questions may be obtained must depend on the support that is forthcoming for future investigations.

JULIAN BLACKBURN

THE SEA

This Great and Wide Sea

By R. E. Coker. Pp. xvii+325+91 plates. (Chapel Hill, N.C.: University of North Carolina Press; London: Oxford University Press, 1947.) 27*s.* 6*d.* net.

THIS book covers the range of physical, biological and geological oceanography, from the point of view of a biologist. It gives a number of comprehensive, but not technical, presentations of the basic phenomena of the ocean as a place of life for plants and animals. It is well written and well illustrated by a large number of good photographs, well chosen.

Part 1 consists of five chapters on history and geography, one of which is on oceanography in

America. There are short interesting accounts of the work of Maury, Wyville Thomson, Murray, Hensen, Chun, the Prince of Monaco, A. Agassiz and many others.

Part 2 consists of seven chapters on chemistry and physics, one being on deposits at the bottom of the sea and three on sea water in motion. The leading idea in these chapters is that "the seas, all together, constitute a great dynamical system with an intricate and world-wide mechanism for mixing everything soluble that comes into it". Of course, physical and dynamical processes are not considered in any detail. Many results are quoted from the recent writings of H. U. Sverdrup, and the whole appears to be up to date and accurate. These chapters have many biological references and prepare the way for dealing with conditions under which marine plants and animals live.

Part 3 consists of seven chapters on life in the sea which are packed with information. There are numerous references in footnotes which pay a handsome tribute to Murray and Hjort's "The Depths of the Ocean", now unfortunately out of print. The author states that "life in the sea cannot be treated adequately in a few chapters: volumes are required", and the truth of this is constantly in the mind of the reader; but the tension is eased by the inclusion of a useful summary at the end of each chapter. The author depends, to some extent, upon a historical approach to unfold the theme of each chapter. He takes the reader back to the benthos and nekton of Haeckel, and is obviously impressed throughout with similar classifications of marine animals according to their mode of life. One chapter deals briefly with algæ, diatoms and dinoflagellates as pasturage of the sea, with a passing reference to the growing importance attached to the study of marine bacteria. There are two chapters on plankton. This is described by reviewing members according to the groups or phyla to which they belong. Then follow chapters on the benthos and nekton, the latter concentrating upon fishes with a special paragraph headed "Useful Fishes". There are also interesting references to the methods of capturing fish which fall in line with the equally useful descriptions in earlier chapters of gear used for obtaining plankton and bottom fauna. To complete the survey, turtles, seafowl, sea otters, sea lions, sea cows and whales are included. Altogether, a full programme which may leave the reader a little breathless, but which ought to stimulate his interest in "This Great and Wide Sea".

SOIL FERTILITY

Chemicals, Humus and the Soil

A Simple Presentation of Contemporary Knowledge and Opinions about Fertilizers, Manures and Soil Fertility. By Donald P. Hopkins. Pp. 278 + 5 plates. (London: Faber and Faber, Ltd., 1945.) 12*s*. 6*d*. net.

A FEATURE of the present-day attitude to agriculture is the increasing concern about soil fertility. Formerly this was confined to a limited number of specialists; but the impact of two world wars and the resulting shortages made all of us think about food, and awakened an interest about soil problems in the more reflective. There is an increasing literature about these matters both from the scientific and economic points of view, and Mr. Hopkins' very readable book deals with a section of this field which has perhaps received wider publicity than any other. The late Sir Albert Howard and his followers have for many years advocated certain methods of soil management in which humus maintenance by means of composts plays the chief part, and chemical fertilizers are entirely excluded. It is claimed that crops grown on compost are healthier and of better feeding value than those grown with chemicals and, further, the continued use of inorganic salts damages the soil population. The full thesis of the compost school has never been accepted by soil scientists; but the case has been put forward with such energy and conviction that there have been many converts, while others are left wondering if there may not be another side to the story. It is here that Mr. Hopkins' book fills a real need. Its purpose is to set out for non-specialist readers the evidence for and against fertilizers as fairly as possible. The author is a chemist engaged in the manufacture of fertilizers, so complete detachment can scarcely be expected; nevertheless his case is presented with a skill and moderation not always found in the rival school.

The book is in two parts. The first deals with the broad facts of plant nutrition, and shows that, unaided, most soils are unable to supply sufficient readily available nutrients for high production. Innumerable field trials have proved that fertilizers rightly used can supply this nutrient deficiency. A discussion of the various means of enriching the soil in organic matter follows, and estimates are given to show that all the organic wastes in sight are insufficient to meet the nitrogen requirements of an intensive arable agriculture. The rational procedure, which has the backing of almost all scientific opinion, is to use as much organic material as possible for its humic value and supplement its nutritional content with chemical fertilizers. In the second part of the book the author examines the charges that fertilizers inhibit mycorrhizal development, and exterminate earthworms and bacteria. He concludes that these charges are not proved; although gross misuse of fertilizers, such as the rendering of soil excessively acid by continued heavy doses of ammonium salts, may upset the soil populations. Passing on to the argument that fertilizers cause plant disease, the author points out that there were devastating outbreaks of disease before artificials were in general use. Once again wrong use of fertilizers, unbalanced nitrogen applications, may increase susceptibility to certain diseases. It is on the grounds of the alleged improved food value of produce grown with humic fertilizers that the compost school have gained many adherents. Even here the evidence from official research stations appears to be negative. Experiments that gave positive indications have, strangely enough, not been extensively repeated; while evidence based on the state of health of widely separated communities consuming different diets is not convincing.

Several topics not directly in the line of argument are included; for example, a concise and well-informed chapter on the fertilizers themselves.

Mr. Hopkins' book is written in a much less formal style than the scientific text-book, and he goes to some trouble to dissect faulty arguments. Apart from the public to whom it is addressed, many students who have mastered the elements of their subject might read this book with considerable profit.

Material for Visual Education

By Betty M. B. Harris. Edited by G. Patrick Meredith. (Published for the Film Council.) Pp. 96. (London: *Daily Mail* School-Aid Department, 1947.) 2*s*. 6*d*.

A VISUAL aid may fail through some inherent defect; or the right aid may be used in the wrong place, in which case the fault lies with the teacher. Yet not wholly so, for in the absence of any comprehensive survey of sources of visual material the teacher has too often been obliged to rest content with what he knew was available.

The book under review can fairly claim not only to map out the main areas of visual provision but also, by an ingenious and practical system of indexes, to make everyman his own explorer. Thus the inquirer for, say, a filmstrip on Chinese art is led through successive summaries, each of which narrows his field of search, until he arrives at the address of the organisation most likely to meet his need.

This book will be much thumbed and deserves a more substantial binding. Regarded from the point of view of a work of reference, the appendix, which deals more generally with the theme of visual education and with the work at University College, Exeter, might well have awaited the separate and more detailed account which is promised in the text.

It is to be hoped that there will be future editions which will expand the work where necessary and keep it up to date. In which case, again for ease of reference, pages of advertisements should not be interspersed with the text. The pertinent reminder, in the Films Section, that "inclusion in a catalogue is no guarantee of merit", might well be transposed to the preface.

ERIC LOWE

They Live in the Sea

By Douglas P. Wilson. Pp. 128 (90 plates). (London and Glasgow: Wm. Collins, Sons and Co., Ltd., 1947.) 12*s*. 6*d*. net.

THIS book of most beautiful photographs cannot fail to delight. To the biologist it is fascinating, and to those with only a superficial interest in natural history, or little appreciation of natural beauty, it is a revelation. From such a collection it is difficult to select individual pictures for special mention. The most dramatic are the series showing a Portuguese man-of-war capturing and feeding upon a wrasse, and that showing a cuttle-fish securing a prawn after a first attempt successfully dodged by the prospective victim. But the author is not content with the dramatic only, and follows up the studies of *Physalia* with an excellent photomicrograph of nematocysts discharged and undischarged, and an enlarged view of the gastrozooids digesting the fish. No less striking are the studies of the strobilation and release of medusæ by the scyphistoma of *Aurelia*. Some of the most beautiful pictures are those of semi-transparent animals, where the author has been highly successful in showing the translucency of sea-anemones, alcyonarians, prawns, ascidians and larval forms. The text, avoiding technicalities and Latin names, gives a clear and accurate account of the animals portrayed in the plates. A short chapter deals with dredging and trawling; and any biologist who has worked at Plymouth during the last half-century will recognize with pleasure the characteristic portrait in Plate 25. From this author we expect outstanding pictures, and we are not disappointed.

L. HARRISON MATTHEWS

Light, Vision and Seeing

A Simplified Presentation of their Relationships and their Importance in Human Efficiency and Welfare. By Dr. Matthew Luckiesh. Fourth printing. Pp. xiv+323+16 plates. (New York: D. Van Nostrand Co., Inc.; London: Macmillan and Co., Ltd., 1946.) 25*s*. net.

IN this book will be found a popular and readable account of numerous problems relating to lighting and vision. A large number of important psychological and physiological experiments carried out under the author's direction are reported in simple terms, with very clear diagrams illustrating the meaning of the results in a practical form. After reviewing the difficulties presented to the human eye by the needs of reading and innumerable everyday activities, the author proceeds to consider eye-defects, and the practical problems and history of artificial lighting. Then he studies the influence of various factors in making objects visible: intensity of illumination, relative size, brightness contrast, eye movements and the time required for seeing, visual acuity and its measurement by test charts. Later he considers the physiological effects of seeing, practical problems of lighting, various common illuminants, colour discrimination, glare and other important matters, including the relation of safety and efficiency to the ease of seeing. At the end of the book he includes answers to one hundred common questions about vision, light and colour.

The industrial psychologist and lighting engineer will find this book a mine of valuable information and practical help about problems of applied science as far as vision is concerned.

R. W. PICKFORD

Molds, Yeasts and Actinomycetes

A Handbook for Students of Bacteriology. By the late Dr. A. T. Henrici. Second edition, by Dr. Charles E. Skinner, Dr. Chester W. Emmons and Dr. Henry M. Tsuchiya. Pp. xiv+409. (New York: John Wiley and Sons, Inc.; London: Chapman and Hall, Ltd., 1947.) 5 dollars.

AS a short and readable handbook for the many different categories of student and research worker concerned with mycological problems, whether industrial or medical, this new edition of Henrici's well-known book should prove of real value. It is up to date—in so far as this is possible at a time when the whole subject is in a state of rapid advance, with the concomitant production of a vast new literature. Indeed, a more or less complete re-writing of the earlier edition has been necessary in order to do justice to the many exciting discoveries of the past decade. The aim, like that of the earlier edition, has been to provide bacteriologists with some of the knowledge which they ought to possess concerning other groups of parasitic and saprophytic micro-organisms, that is, yeasts, moulds, and actinomycetes.

The result is a book which, within its set limits, should prove useful to many. The stress, of course, is on the medical and industrial applications of mycology. In fourteen chapters, a wide range of topics is discussed and admirably illustrated. These include general dissertations on the structure, classification and methods of study of moulds, yeasts, etc., and particular accounts of fungi causing diseases of man and animals, and of use in industrial fermentations, etc. Yeasts and actinomycetes are accorded a fairly full treatment, and a final chapter is devoted to antibiotic substances.

THE NEW ERUPTION FROM HEKLA

By Dr. G. W. TYRRELL
University of Glasgow

AFTER a hundred and two years of quiescence, Hekla broke out in violent eruption at about 6.50 a.m. on March 29, 1947. Three days later, Dr. Jòh. Áskelsson, of the High School of Reykjavik (University of Iceland), sent me a fragment of the new lava, which I described in a 'letter' in *Nature* (May 10, 1947). The lava proved to be identical with previous Hekla lavas and, indeed, with the general run of Recent lavas in southern Iceland. I got into touch with Dr. Trausti Einarsson, also of the University of Iceland, who, as well as Dr. Áskelsson, had been a research student in the Geological Department of the University of Glasgow. Dr. Einarsson sent me numerous newspaper cuttings (in Icelandic), two long letters descriptive of the eruption, and a new book, "Hekla", by Gudhb. Kjartansson (167 pp., with numerous illustrations and a map), which was published as a Yearbook of the Icelandic Touring Association, 1945. What follows is mainly derived from Dr. Einarsson's letters.

Numerous observers have been following the eruption, and relays of workers have made it possible to record its phenomena hour by hour. In addition to J. Áskelsson and T. Einarsson, G. Kjartansson (author of the above-mentioned book), S. Thorarinsson, P. Hannesson, T. Tryggvasson and S. Sigurdsson among Icelandic geologists, N. Nielsen and A. Noe-Nygaard from Denmark, A. Gavelin from Sweden, and L. Hawkes of Bedford College, London, have visited Hekla and have worked on various aspects of the eruption. Samples of rocks and gases are being analysed in the State Research Laboratory, and some geophysical work is being done. Kjartansson and Tryggvasson will collaborate in a petrological and chemical study of all Hekla lavas.

According to Einarsson (letter of May 26), the main events of the eruption were as follows. It began a little before 6.50 a.m. on March 29, apparently without any preliminary symptoms. A considerable earthquake shock was felt as far away as Reykjavik; but, curiously enough, few noticed it in the region immediately surrounding the volcano, and people in farms only 9 km. distant were not even awakened. No serious damage was done by this shock. The sky was clear and, immediately after the shock, an enormous column (of steam and ash ?), more than 20 km. in height, was seen from Reykjavik. A north wind was blowing which carried the erupted material southward. The inhabited regions of Fljótshlidh and Eyjafjöll were enveloped in complete darkness and soon covered by a layer of pumice and ash three inches thick. A continuous wall of black 'smoke' 10 km. high extended far out to sea.

Fig. 2. Hekla, morning of March 29, 1947, looking south-south-west. Photo by V. Sigurgeirsson, Reykjavik

At 10 a.m. Einarsson and others took aeroplane to the scene and found black clouds of ash being whirled out with tremendous force from a number of craters along the whole length of the volcano. (Hekla is an elongated ridge rather than a cone; see map, Fig. 1.) The mountain itself seemed a trifle in comparison with the enormous columns of ash (see Fig. 2). Lava had even then reached the plateau surface to the north and east of Hekla, and it was ascertained later that the lava was actually issuing from the whole length of the ridge. Streams of black mud were also being generated, no doubt by the mixture of thawed glacier ice and snow with ash; and on the first morning of the eruption an enormous rush of water from the north-west slope of the volcano caused a flood in the River Ytri-Rangà—a common feature of previous eruptions (see map).

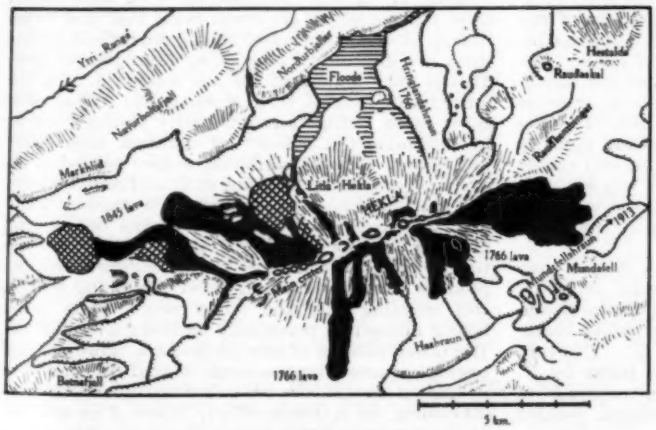

Fig. 1

Einarsson measured the temperature (950° C.) of a lava flow from a low-level crater on the south-western flank near Nafrholt. The lava here is welling out quietly as a fountain, and is very viscous. "With good asbestos insulation", says Einarsson, "it would be a simple matter to walk on it in the crater

itself. You can stand on the rim in the crater and reach the lava with a stick."

At the same time furious explosions were taking place in the higher craters. In a few days, however, the activity greatly diminished, especially at the northern end of the ridge. Lava ceased to flow from the crest, but flowed continuously, and still flows, from the low south-western craters. The extent of the new lavas on April 8 is shown on the map (in solid black).

While all activity ceased in the north-east and lava ceased to flow from the crest, violent explosions continued at the summit crater and the high-level craters of the south-western flank. In many of the explosions large blocks were thrown to a height of 500–600 m., and the detonations were so loud that it was difficult to sleep in Thórsàdalur, 20 km. distant from the volcano. Again, at shorter distances, the same explosions did not seem to be so loud owing to some acoustical trick.

Flashing arcs of light were a common sight. Ash was only occasionally blown out in quantity and was harmless at first. Nevertheless, if it continued into the haymaking period serious effects would follow. The pumice fall in Fljótahlidh and Eyjafjöll, however, caused the evacuation of some farms.

With the exception of the initial shock, notable earthquakes have not occurred in the vicinity of the volcano; but in the district of Ölfus very heavy shocks occurred on May 19. Hot-spring activity was greatly augmented, and every quiescent pool in a long, narrow strip of country became a violent geyser of dirty water.

In a further letter (July 17), Dr. Einarsson states that the eruption goes on. Lava still flows uninterruptedly (presumably from the south-west craters) without any noticeable decrease of volume. The eruption of ash, however, has diminished to insignificant proportions, and is doing no harm. A curious result is that "during one period of heavy ash fall and dry weather the sheep got the usual 'eruption disease', which is caused by fluorine, but when a rainy period set in they somewhat recovered. The disease is, so far, not very serious."

With regard to the magnitude of the eruption, Dr. Einarsson is of opinion that the present eruption is at least as great as that of 1845, and will probably prove ultimately to be greater.

I visited Copenhagen on a lecture tour during October, and gained a few further details from Prof. A. Noe-Nygaard, and saw his very fine film of the eruption. The new crater on the summit is 300–400 m. in diameter, and has reached a height of 100 m. The orifice which continues to emit lava at the south-western end of the ridge is a fissure with six or seven small craters, very clearly seen in the film. The line along which the new eruption has taken place is situated some distance to the west of the still visible rent from which the eruption of 1845 was emitted. The film shows the minute details of the over-rolling and incorporation of solid lava-blocks by the liquid lava, and of auto-intrusion.

The accompanying map is re-drawn from a map published in the newspaper *Morgunbladid* (April 15, 1947) by Kjartansson, Hannesson and Thorarinsson. It shows the extent of the new flows (solid black) with additions (hatched) up to May 20. The very fine photograph of the eruption was taken by V. Sigurgeirsson of Reykjavik on the morning of March 29, the first day of the eruption, looking south-south-west.

Addendum. In a recent letter (November 9), Dr. Einarsson states that lava is still flowing in undiminished quantity from the crater on the south-west. Explosive activity ceased at the end of June. The lava is becoming very rich in gases and is more fluid than before, while the temperature remains constant.

CORN (*ZEA MAYS* L.) GENETICS IN THE UNITED STATES

By GORDON HASKELL

Connecticut Agricultural Experiment Station, New Haven, Connecticut

MAIZE (*Zea Mays* L.), or 'corn' as it is known throughout North America and so called throughout this article, is by far the largest crop grown in the United States, having many different uses ranging from animal food, a basis for the manufacture of nylon, to its use as table sweet corn. It is therefore not surprising that much time and money are spent investigating the corn plant. In addition to its economic importance, corn possesses a number of properties making it useful experimental material for cytologists, geneticists, plant breeders, and for evolutionists interested in the origins of cultivated plants. Some aspects of the genetic and breeding problems now attracting attention and an indication of lines of approach for solving them will give a brief indication of current work with this plant in the United States.

The problem drawing very wide attention at the moment is how to estimate combining ability of inbreds used in the production of hybrid seed corn. Modern strains of corn used commercially throughout the United States are known as 'double crosses' derived from crossing four inbred pure lines thus: $(A \times B) \times (B \times C)$. It is therefore highly important to know what open-pollinated varieties still in existence are more likely on inbreeding to give rise to inbreds which have the ability to produce the largest amount of hybrid vigour when crossed with other inbred lines. Jones, Singleton, Nelson and Hayes believe that it is best first to obtain the inbreds themselves possessing good plant characters before determining their potency in various combinations with other inbred lines, their principal argument being that it is no use having excellent hybrids if the inbreds which go into them are difficult to keep going from year to year. This is very important for the seedsmen. On the other hand, Jenkins, G. F. Sprague and colleagues at Iowa State College advocate early testing, that is, to determine the potency of different lines before inbreeding. General combining ability defined as the average performance of a line in hybrid combinations, and specific combining ability with a standard tester line, may both be used as pre-testers before commencing the inbreeding programme. Early elimination of varieties which pre-testing indicates will not produce inbreds possessing good combining powers, and selecting only the upper range, saves much labour inbreeding potentially unproductive material. The disadvantages of this method are, however, that testing requires a large amount of land and that open pollinated lines of corn are so heterozygous that often inbreeding has a drastic effect. There is no stability of the numerous genes segregating until after several

generations of inbreeding, so that the combining ability of the derived inbreds has still to be estimated later, and this will vary *intra se*.

There has been some attempt to predict the value of inbred lines, using a cytological method. It now seems probable that the dent corn of the Mid-West was originally derived from hybridization between southern prolific dents and northern flint corns. Inbreeding mid-western dents releases variability, which manifests itself in plant habit and ear characters. The problem is to determine what two inbreds give the greatest heterotic effects and to determine whether this is predictable on the basis of the characters of the derived inbreds. It has been known that the greatest jumps can be expected from inbreds derived from open-pollinated varieties which differed widely in plant type and source of origin, for example, the northern corn belt hybrids which are dent corn crossed with flint corn, and the sweet dent silage hybrids. Inbreds derived from mid-west dents range from flinty types to extreme dent kinds. Longley had previously studied the positions of corn chromosome knobs, and Brown is now applying this method to inbreds derived from mid-west dents; those inbreds having knob numbers of approximately two are most like the northern flints, while those with higher numbers are more like southern dent corn. One would therefore expect that hybrids between inbreds of high and low knob number would be better than those between high knob numbers or between low knob numbers. Therefore a combination of seed characters and knob number of inbred lines may assist in reducing their number for testing and may indicate what inbred combinations are likely to prove most profitable.

The present basis for the hybrid corn industry has depended upon inbreeding different strains of open pollinated varieties to obtain the inbred lines, and then to make two sets of F_1 hybrids and cross these together to obtain the double-cross commercial hybrid. To obtain satisfactory homozygosity the lines may require not less than four or five years of selfing, and even then the amount of inbreeding depends upon how far the eye can judge the purity of such lines. Thus any method that could (*a*) short-cut the inbreeding programme and (*b*) give a more dependable amount of homozygosity would be valuable. Such attempts have been made. For example, a number of workers have investigated the effect of using pollen, sufficiently damaged by X-ray treatment as to be incapable of effecting fertilization, yet still viable and capable of growing down the style. By so doing it was hoped to stimulate haploid development of the egg cell without fertilization. So far results have been negative, and now Lindstrom is making a new attack on the problem, first by the application of numerous chemical substances to the silks to induce parthenocarpy, and also by exploring the possibility of using a method already successfully applied to *Linum*, namely, the selection of polyembryonic seeds in the hope that some of the embryos will be haploid. The haploid set of chromosomes can then be doubled with colchicine, immediately giving rise to autodiploid plants. Such plants would have much value in future investigations. It would seem, however, that these short-cuts must at the moment be considered more in the light of academic interest than of economic value.

Another problem of corn breeders is that even though they may establish inbred lines which carry with them the required amount of homozygosity to allow their use in hybrid combinations producing hybrid plants uniform for such quantitative characters as maturity, height and ear length, major mutations constantly occur. Jones first thought that degenerative changes were associated with the change in combining ability of the inbred lines; but now it seems likely that invisible mutations controlling combining ability continually occur in them, and are not readily detected until the inbreds are hybridized, either to inbreds from different sources or back to the original inbred lines. Hence the major degenerative changes only acted as stimulants for testing and so disclosing the cryptic mutations, but were not themselves responsible for such changes.

There are quite a number of general selection experiments with various types of corn for characters under multifactorial, or polygenic, control. One group of experiments refers to the selection of certain chemical constituents of the corn kernel. For example, at Iowa State College work directed towards the production of inbreds and hybrids of waxy corn is comparable with methods used for the production of field corns. The waxy gene corn was successfully utilized during the war years as a substitute for tapioca. Similarly, the oil content of kernels has been increased by the selection of strains with large embryos, hence increasing the value of corn feed. Many straight selection experiments with corn are being practised at the northern edge of the corn belt to obtain hardy lines which are also adjusted to the altered day-length of this area. Thus Minnesota, Wisconsin and Maine Agricultural Experiment Stations have developed crosses between flint and dent corn to give flint/dent hybrids with good plant and ear characters yet sufficiently hardy for the northerly areas. At the same time the Central Experimental Farm is selecting good flint lines for Ontario. So the corn-growing area is gradually moving farther north.

Another problem attracting much attention is the study of principles underlying the ability of some lines of corn to germinate after remaining under cold soil conditions while other lines are severely damaged. These investigations are being carried out at the Minnesota, Iowa and Connecticut Agricultural Experiment Stations and elsewhere. Hayes and his colleagues have now investigated the genetical basis for such phenomena, having established that different lines have different resistance to cold treatments. Haskell has determined the hardiness of some of the Connecticut inbred sweet corn lines and found, contrary to expectation, that they are not always worse than the behaviour of the starchy control under the conditions of the experiment. In general, although the usual experience of breeders is that sweet corn seed over a wide range of conditions germinates more poorly than that of other types of corn, this may not be due directly to the *Su/su* relationship, but to closely linked genetical factors and also to easier damage of the wrinkled sweet kernels during drying and threshing in the seed factories, which later allows pathogenic soil organisms to penetrate more easily. Commercial seed companies now use cold-room tests for determining the value of their corn strains. This is important, as the effects of the continental climate of the United States, with extreme hot and cold rapid changes in the soil at germination time, require strains to be adapted to such conditions. In general, it has been found that conditions of 50° F. for 8–10 days are most expedient for test purposes, and probably also as a selection criterion for a breeding programme.

Since the production of the first Golden Cross sweet corn by Glen Smith, hybrid sweet corn production has greatly progressed, with the adaptation of special inbreds and their hybrids for particular areas of the States. In view of the fact that sweet corn is primarily a canners' crop, great stress has been laid on uniformly maturing hybrids, and sweet corn breeders have established a complete series of early, medium and late inbreds and *single cross* ($A \times B$) hybrids, so that growers can use mechanical pickers and sweet corn canneries can work over a spread of time. Compared with field corn, sweet corn breeding has been to some extent simplified because single crosses may be used, the prices of seed being higher than for field corn. With the maturity range established, attention is now largely focused on quality testing. This is an elusive item to estimate, and although the sugar content of kernels is of some value, still the only satisfactory method is by tasting, usually in the raw state, rather than by any laboratory tests, such as estimating translucency, or puncture testing for pericarp tenderness. By experience, sweet corn breeders generally agree upon what good-quality sweet corn should taste like. Attention is also paid to the behaviour of the different sweet corn hybrids under freezing methods, since more ears are sent to market on ice each year.

Many of the northerly experiment stations in the United States and also in Canada are selecting early, hardy sweet corn lines. Owing to the sudden killing frosts towards the end of the season, these strains must be so adapted that seed is sufficiently matured for safe picking and for artificially drying before being otherwise killed on the ear. There have been various determinations of the optimum drying conditions. In general a temperature of 100° F. for three days is often sufficient to bring down the moisture content of the seed in the hard dough stage to a level permitting safe storage. Besides pushing sweet corn production northwards there is also an active attempt to spread sweet corn for canning and direct eating into the Southern States, where white, dent corn is now grown for roasting. The resistance to the use of yellow sweet corn seems largely to be based on ignorance of the southern farmers and to the fact that no hybrid strains have been adapted to their climatic conditions, many northern hybrids being unable to meet the severe attacks of the corn ear worm in this region. These particular problems are now being overcome.

For a long time much attention has been paid to the history of the corn plant, concerning the origin of present forms and how it spread through the North American continent with the movements of the early American Indians. In addition, corn is one of the few cultivated plants the prototype of which has so far not been discovered growing wild in Nature, and its ear, so different from the female inflorescence of other members of the Gramineæ, offers interesting material for investigation. There is a division of opinions as to the source of corn. Mangelsdorf, Reeves and Merrill believe corn is a specialized product of selection by early American Indians of a grass that grew wild on the American continent. The present site of this prototype is still to be located, but evidence points to South America as the primary centre of origin. Teosinte (*Euchlæna*) is a hybrid between maize and *Tripsacum*, and is not a progenitor of maize, although where teosinte grows as a weed in Mexican corn fields there is a high incidence of hybridization. On the other hand, Edgar Anderson suggests that besides genetical indications, there are several pieces of ethnological evidence which suggest Burma as a possible source for the origin of corn. He has enumerated several pieces of circumstantial evidence showing similarities between Asiatic and Pre-Conquest civilizations. In addition, it is known that other members of the Tribe Maydeæ, to which maize belongs, occur in tropical Asia.

Similarly, views on the origin of sweet corn are at variance, Mangelsdorf believing that the *su* mutation for sugary kernels has occurred spontaneously in different areas throughout America, and has been adopted by local farmers in those areas for specialized use, such as making fermented liquors; while Edgar Anderson conceives that the sugary mutation has gradually spread northwards across the American continent from a more southerly point of origin. At the present time, although much light has been shed on these problems, the origin of corn still awaits a final solution.

Acknowledgments are made to the Agricultural Research Council of Great Britain for funds allowing the writer to visit the corn belt of the Mid-Western States.

ASPECTS OF THE LOCOMOTION OF WHALES

By R. W. L. GAWN, O.B.E.
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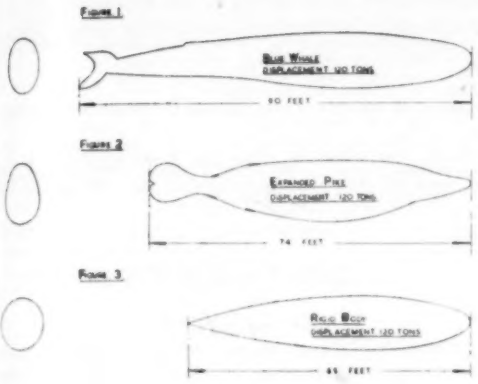
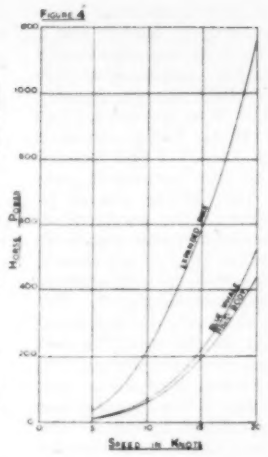
REPORTS of the 1946–47 British Scientific Expedition to the Antarctic Whaling Ground have revived interest in the locomotion of whales and other marine mammals and fish. Some interesting studies and information on this subject already exist, but many of the reports of speed recorded in the past have proved rather conflicting in character. Care appears to have been taken on the recent Expedition to obtain reliable observations, and it now appears to be substantially confirmed that a blue whale can attain a speed of 20 knots for a short burst of about ten minutes duration, and maintain a speed of 14½ knots for two hours and probably longer. A killer whale may obtain slightly higher speeds.

The shape of fish and marine mammals has naturally been an inspiration for the forms of underwater bodies for a great many years, and investigations have been carried out accordingly. It is pertinent to examine whether any lessons can be learnt from the whale and other marine creatures that can contribute to faster speeds of submarines and other underwater locomotive bodies.

Particulars of the shape of fish and marine mammals were given so long ago as 1919[1]. The form is not unlike that of a good streamline body. A blue whale of normal proportions may be about 90 ft. long and as much as 120 tons weight. The shape is illustrated in Fig. 1.

In order to propel a smooth rigid body of this size and shape at 20 knots, it is estimated that 520 horsepower would have to be developed by the whale. The estimate for other speeds is shown in the curve in Fig. 4. This curve is deduced from measurement of the resistance of models of smooth rigid bodies towed in water. These bodies form eddies which account for a fair proportion of the total power. It may be argued that the whale may succeed in avoiding eddy resistance by the natural flexure of the tail and





after-body generally. On the other hand, the unevenness caused by the fins and the fluting of the body will result in some increase of the horse-power required as compared with a smooth rigid body. It is also assumed that the propulsive efficiency of the tail of the mammal is equal to that of the best marine propeller, namely, 75 per cent. Here again it may be argued that a higher efficiency should be assigned because the tail of the whale is a comparatively large surface and moves at a slow rate. It may therefore be assumed that the rate of change of momentum to propel the whale is achieved by discharging a large quantity of water astern at a comparatively low speed. As a result the kinetic energy wasted in the race will be reduced. The energy so lost may be only about 10 per cent of the total, which indicates a possible 90 per cent for the efficiency. In assessing an efficiency of 75 per cent, I have, however, been guided by the results of a few experiments on flapping fins that have been carried out abroad.

It is of interest that the horse-power curve so obtained is very similar to that previously recorded in *Nature* by Prof. A. Krogh[2], although the method of estimate is different. In particular, the previous estimate assumed an efficiency of 100 per cent for the 'propeller', which is clearly inadmissible because of the loss due to kinetic energy in the water discharged sternwards.

If the curve in Fig. 4 truly represents the performance of the live whale, the developed horse-power per ton is 4·35 at 20 knots and 1·75 at 15 knots. This expenditure of power in relation to the weight is comparatively large and implies that the whale attains its remarkable speed by exertion of large power rather than by any special virtue in shape of body or smoothness of skin. There is no known measurement of the power which a whale can develop. Some approach to the problem can, however, be obtained from physiological researches. This question has been discussed by Gray[3] and Barcroft[4]. It appears that 0·01 horse-power per lb. of muscle is developed by athletes and dogs. There seems no reason for believing that whale muscle is more efficient as a machine than the muscle of other mammals. It also seems established that about 40 per cent of the weight of a whale consists of muscle. On these assumptions it would appear that the whale can develop 9 horse-power per ton of total weight. Thus the estimate of horse-power given in the curve is not inconsistent with the power obtainable from the muscles of the mammal.

It may be suggested that the surface friction resistance of the whale is less than that for turbulent flow past a rigid body. Taking a favourable view, the flow may be laminar. The horse-power would then be much less than that given in Fig. 4. While there is no definite evidence to prove or disprove this, such an explanation of the speed of the whale is unnecessary, since the horse-power mentioned above is not beyond that to be expected from a creature of 120 tons weight. There is also independent evidence[1] that great power is, in fact, developed by the whale; for example, a Sei whale is reported to have towed a vessel at 12 miles per hour, and a blue whale to have towed a steamer at 7 miles per hour, although the engines were working full speed astern.

A point which must not be overlooked and on which further information would be valuable is the means by which the whale avoids excessive rise of temperature when developing this large power. Assuming 22 per cent efficiency for the muscular engine, then the rise of temperature of the whale for a 10-minute burst of speed at 20 knots may be about 3° F., unless arrangements are made for dissipating the heat.

It should be added that the estimate of horse-power in Fig. 4 assumes that the whale is so deeply submerged that surface waves are not produced. It is understood that a whale normally proceeds at about three fathoms submersion and frequently on the surface. If the whale is proceeding on the surface at low buoyancy, it is estimated that the horse-power at the top speed would be treble that in the figure. If at three fathoms depth, the increase would be about 50 per cent. These estimates are deduced from experiments that have been made in water on models of shapes analogous to the whale.

A fish for which there is reliable information is the pike[5]. Experiments were carried out in the Hamburg Ship Model Experiment Tank. An anæsthetized pike was towed in the experiment tank, and the resistance measured at various speeds. The resistance was much greater than for a smooth solid body of similar shape. The oil exuded by the fish mollified the unevenness of the skin due to the scales to some extent, but the reduction in skin friction was inconsiderable. Observations of the speed of pike gave maxima of 6–8 knots, the mean speed being 5 knots. The effective horse-power per ton is considerable and varies from 26 at 8 knots for a large pike to 53 at 6·8 knots for a small pike. The tests established that the speed and form of individual fishes of the same type vary considerably. So far as they go, the experiments do not support the theory that fish have abnormally low surface friction.

Possibly anglers whose equipment has suffered in the struggle with a lively victim might hold the view

that the pike tested in the Hamburg Tank were rather unenterprising. I am not an angler, but I have observed fish and have been impressed with their capacity for developing high acceleration over short periods. This led me to take the opportunity when in Hamburg before the War to discuss the experiments on pike with Prof. G. Kempf, director of the Hamburg Experiment Tank. I feel assured that the investigations on the pike were most carefully conducted, and that the results should be accepted as an accurate record of the performance of the species tested. Very complete particulars were taken of the shape of the pike. Fig. 3 indicates a pike of weight 9·05 lb. expanded to 120 tons to compare with the blue whale shown in Fig. 2. The horse-power for the pike as measured in the Hamburg Tank has also been expanded by the usual method adopted in ship-model experiments to compare with a whale. It has again been assumed that the propeller efficiency is 75 per cent. The horse-power curve for the expanded pike is shown in Fig. 4. The horse-power considerably exceeds that for the whale. This naturally casts a doubt on the estimate for the whale, but it may also reflect the wide difference between a small freshwater fish and a large deep-sea mammal. It is of interest to compare the performance with that of a rigid body of good design. Such a body is shown in Fig. 3. A model of this body has been tested both in water at the Admiralty Experiment Works, Haslar, and also in a wind tunnel. The horse-power curve deduced from the results is also shown in Fig. 4. The results compare favourably with the expanded pike and the whale.

Porpoises have been credited with a speed of 25 knots. This is a very high speed for a small creature to maintain for any length of time. Proceeding in the same way as for the whale, it is estimated that 14 horse-power would be required at this speed for a porpoise of 0·16 ton weight. This represents the high figure of 87 horse-power per ton. If, however, the speed is 20 knots, the horse-power per ton is reduced to 43, which is comparable with the pike.

It would be of great interest to ship designers and more particularly those associated with the design of submarines and other underwater bodies to obtain some reliable information concerning the whale. The day may be far distant when the towing power developed by the muscles of the whale can be accurately measured, but it would be of considerable interest to have the views of physiologists as to the muscular power. It should not prove a difficult matter to obtain a reasonably accurate measurement of the towing resistance of whales at various speeds. This is recommended for consideration by those responsible for organising future whaling expeditions. The whale differs from other marine creatures in that the tail is horizontal and moves vertically. Some information on the size of the tail and fins, and the method by which the whale dives and surfaces and maintains stability for course and depth, would also be of interest. If the particulars of the fin surfaces could be obtained, there is scope for wind tunnel investigations on the whale similar to the interesting tests on fish stability carried out by Harris[6].

I wish to express my appreciation of the help generously given by Dr. R. A. M. Case, of the Royal Naval Physiological Laboratory. Dr. Case was the leader of the 1946–47 British Scientific Expedition to the Antarctic Whaling Grounds, and he has provided particulars of the size and speed of the blue whale and has very kindly given references to the physiological work quoted in this article.

[1] "Experiments in a Wind Channel on Elongated Bodies of Approximately Streamline Form". *R and M* 607, March 1919 (Advisory Committee for Aeronautics).
[2] Krogh, A., *Nature*, **133**, 635 (1934).
[3] Gray, J., *J. Exp. Biol.*, **10** (1933).
[4] Barcroft, J., "Features in the Architecture of Physiological Function" (Camb. Univ. Press, 1938).
[5] *Z. Ver. Deut. Ing.*, **79**, No. 33.
[6] Harris, J. E., "The Role of the Fins in the Equilibrium of the Swimming Fish. I. Wind Tunnel Tests on a Model of *Mustelus canis* (Mitchell)", *J. Exp. Biol.* (1938). II. "The Role of the Pelvic Fins", *J. Exp. Biol.* (1936).

OBITUARIES

Dr. C. C. Hurst

CHARLES CHAMBERLAIN HURST, whose death occurred on December 17, was born in 1870 and educated at Wesley College, Sheffield. As a member of the family firm at Burbage, near Leicester, he was early brought into contact with horticultural practice and problems, and at the same time afforded opportunity for embarking on the investigations with which his name is connected.

Hurst was working at cross-fertilization in orchids when Bateson's papers in the *Journal of the Royal Horticultural Society* in 1900 brought the news of Mendel's discovery to the English-speaking world. Hurst at once got into touch with Bateson and started experiments with rabbits and poultry with the view of testing Mendel's conclusions. With rabbits he was able to confirm the results of Cuénot on coat colour in mice, and his extensive series of crosses among poultry, an account of which appeared in the Reports to the Evolution Committee of the Royal Society in 1904, added considerably to our knowledge of the genetics of the fowl. Better known was his demonstration of the recessive nature of chestnut in horses, which provoked much discussion when read before the Royal Society.

Perhaps the piece of work for which Hurst is most likely to be remembered is his paper on the "Inheritance of Eye-colour in Man". Through the careful and laborious collection of data from his native village of Burbage, where he was well known and trusted, he was able to determine the recessive nature of blue to non-blue eyes, and must thereby receive the credit for being the first to demonstrate the Mendelian inheritance of a 'normal' character in man.

Although he had, as it were, strayed for a time in the animal kingdom, he had by no means given up his horticultural interests. His attraction to the genus *Rosa* led him to form an outstanding collection of wild forms from all over the world, and his hybridization work contributed much to the understanding of this remarkable case of polyploidy.

As to many another scientific worker, the outbreak of war in 1914 came as a sad misfortune. In his capacity of a Territorial officer he joined the Army, and at the conclusion of hostilities he found himself unable to carry on at Burbage. Thereupon he entered Trinity College, Cambridge, as a fellow-commoner, and eventually his research work led to the award of the Sc.D. degree by the University. Fortunately he had been able to shift much of his collection of rose species to the Botanic Garden at Cambridge and was able to carry on his study of the genus, cytologically as well as in the field. Meanwhile he wrote his book on "Creative Evolution", an

attractively illustrated exposition of the state of genetics at the time.

Hurst was gifted with fine powers of observation. He had, too, enthusiasm for an idea and a capacity for sustained and pertinacious work, both of which he devoted whole-heartedly to the furtherance of genetical science. That he was somewhat lacking in critical faculty may have been due to the fact that he had not been through the academic 'mill'. He was fortunate, however, in that his collaboration with William Bateson brought him into close touch with a critical mind of the highest order. Without that influence he might have become more the gifted amateur than the solid contributor to the science that claimed his interest. R. C. PUNNETT

Dr. Stephen Miall

THE death of Stephen Miall on October 12, 1947, in his seventy-fifth year, inflicted on chemists an irreparable loss. Yet, paradoxically—and he enjoyed a paradox as much as any man—Miall was not a chemist at all. Of Yorkshire stock, with an inherited leaning towards science or, at any rate, towards natural philosophy, he was articled to his uncle, partner in a London firm of solicitors, when nineteen years old. He took prizes and the degree of LL.D., but right up to his end many hundreds of industrial chemists in Great Britain and elsewhere must have been entirely unaware that 'Doctor' Miall—that kindly, erudite, sociable, eclectic personality emerging almost as clearly from his editorials in *Chemistry and Industry* as from his twinkling eyes and grave smile, when one met him—was, in fact, a doctor of law and not of science at all.

Nevertheless, his contact with the chemical industry dates back to the beginning of the century, when he became very much concerned with the manufacture of white lead and, as a result, with the humanitarian aspect of that technology; that is, with lead poisoning as an industrial hazard, in connexion with which he more than once attended an International Labour Conference or discussions at the International Labour Office. Some time after qualifying in law, he also took a London B.Sc.

Miall's editorship of *Chemistry and Industry* began in 1922, and he remained at this post until two and a third years before his death. During his retiral, his health was not what his many friends and admirers had hoped for him; but he often found time and ability to join them at his favourite local haunt or in the Savage Club, where his wisdom, knowledge and slightly mordant wit, almost always mitigated by a cheerful grin, made him a most popular as well as a distinguished and unmistakable figure. He had great erudition, though it was wide rather than deep. He used it to illuminate both his attractively and, indeed, uniquely written editorials and his very deliberate but equally unique conversation, an art the apparent passing of which he always deplored. Homer and Horace as often furnished his reading—and his quotations—as did the English classics: he knew them all intimately.

Among his activities, besides his editorial work, have been the compiling of a light-verse anthology, "Poets at Play", active participation in the Society of Yorkshiremen in London and 'The Catalysts' Club', and the writing and editing, jointly or alone, of books designed to strengthen the links between chemists, especially industrial chemists, and the non-scientific world, particularly that part of it concerned with the arts. It is not surprising that his death will be mourned by a large and varied collection of individuals, some of whom may never have met him and will therefore not experience the strong sense of personal loss that is common to all those who had. A. L. B.
P. J. V.

Prof. G. C. MacCurdy

NEWS has been received of the death in a road accident, at the age of eighty-four, of Dr. G. G. MacCurdy, the well-known prehistorian and professor emeritus of Yale University.

George Grant MacCurdy came from the southern branch of a well-known New England family, which had retained a close contact with Great Britain. He was a graduate of Harvard and of Yale, and from 1894 until 1898 he followed postgraduate courses in anthropology at the Universities of Paris, Berlin and Vienna. During 1900–31 he was successively lecturer in anthropology, assistant professor of prehistoric archæology, and research associate with the rank of professor at Yale University, and during the whole of this time he was also curator of anthropological collections. In 1921 he took a leading part in the foundation of the American School of Prehistoric Research, and was its first director, a post he held until last year, and in which he was supported by the collaboration of his wife, Janet Glenn Bartlett, whom he had married in 1919. His tenure of the directorship was marked by the organisation of many successful vacation tours in Europe, and by participation with various British funds and institutions in prehistoric excavations in Iraq, Palestine and Bulgaria.

This work was very close to Dr. MacCurdy's heart, and he was justly proud of the leading part played by a member of his own school, Theodore D. McCown, in the discovery of the new human fossil, Mount Carmel man, during 1931–32. His disinterested courage and vision in thus committing his school to a participation in field-work in which he himself was not free to take an active part has been of incalculable value to the advancement of prehistoric studies in the last twenty years, and will be his lasting monument.

His friends and pupils will cherish an affectionate memory of a distinguished scholar with a singularly kindly, gentle and modest personality.

D. A. E. GARROD

Prof. Max Planck, For Mem.R.S.

PROF. J. R. PARTINGTON writes: I knew Planck as professor of theoretical physics in the University of Berlin. His course was spread over more than one year, and I began with the last and most difficult part, comprising thermodynamics, the theory of heat, relativity, and quantum theory, next year taking the first part, which dealt with general mechanics and the theory of deformable bodies. The lectures began at 9 a.m. in the University building in Unter den Linden; they were very well attended, and there were always many standing around the room. As the lecture-room was well heated and rather close, some of the listeners would from time to time drop to the floor, but this did not disturb the lecture. The windows were all closed, partly because of the intense cold and partly because of the noise of the changing of the guard outside. There was also an

exercise class, conducted by Planck himself, which nominally began at 8 a.m., but usually about 8.15. The medical lectures began at 6 a.m., so the University building was well aired by that time.

Planck lived some distance away, in the Grünewald, and travelled into Berlin by the Stadtbahn. His train often ran parallel to the one I was in from Charlottenberg for a short distance, and Planck could be seen, in a compartment filled with clerks and shop-girls, conning his notes in preparation for the lecture. In the lecture itself he used no notes. He began by writing down a simple equation, but would soon be in the middle of a Fourier series, or, as in the theory of elasticity, a complicated equation which seemed to develop itself. He never made a mistake and never faltered. Very rarely he would take his notes out, look at the board, say "Ja", and put them back again. He spoke in a quiet, audible and pleasant voice, and was the best lecturer I ever heard. He had no mannerisms except one: he had two sticks of chalk before him in parallel arrangement, which he changed about in place from time to time when he was not writing; he had a small reading desk and did not walk about. The mathematics on the board was extremely neat and legible, even when German vector symbols were used, and was set out in systematic order.

Planck's method was to develop the subject logically from general principles, and even when short cuts were possible, he liked to bring each topic into its logical sequence in a general scheme. This made some of it hard going for such as myself. He disliked vector methods, and although he usually wrote the final result in vector form when desirable, the deduction was in terms of Cartesian or polar co-ordinates. He had two favourite sentences. One was "Wir können noch einen Schritt weitergehen", and the other "Jetzt ist die Sache vollständig genügend erledigt".

His lectures on sound included some theory of music, and he gave an extra demonstration with a special harmonium of the natural scale and its necessary modification, "während die praktische Musik, besonders seit der Einführung der Instrumente mit festen Tönen, mit einer endlichen, nicht zu zahlreichen Menge von Tönen auskommen muss".

Planck was just then developing the 'second' quantum theory of continuous absorption and discontinuous emission (which brought in the half quantum of zero-point energy). He was conservative, and a little shocked by Einstein's bold development of the photon. He was keenly interested in the impact of the quantum theory on general philosophical ideas, and shrank from the extreme view that its later phases removed the validity of the law of causality. To the rather pessimistic outlook which came over what was once regarded as the solid foundation of science he was unsympathetic. Theoretical physics had progressed, it is true, but to him it still had a meaning.

Prof. E. N. da C. Andrade writes: "In the last paragraph of my obituary notice on Prof. P. Lenard (*Nature*, December 27, p. 895), I venture to refer to him as 'a dark genius'. Going through some old papers I have just found a letter, dated 1936, from a well-known German physicist whose name I refrain from quoting because of the difficulty, at the present time, of obtaining his permission. It contains the following passage which I have translated except for the actual quotation.

"I have found the following lines in Hebbel which refer there to the Lord God, but apply very well to Lenard in our Heidelberg time:

"'Und aus seinen Finsternissen tritt der Herr, *soweit er kann*'."

We regret to announce the following deaths:

Prof. E. C. C. Baly, C.B.E., F.R.S., Grant professor of inorganic chemistry in the University of Liverpool during 1910–37, on January 3, aged seventy-six.

Prof. P. A. C. Dangeard, member of the Botanical Section of the Paris Academy of Sciences, on November 10.

Dr. C. A. Mitchell, formerly editor of *The Analyst*, on January 5, aged eighty.

Mr. W. H. Pick, of the Meteorological Office, on December 26, aged fifty-seven.

Prof. H. A. Prichard, White's professor of moral philosophy in the University of Oxford during 1928–37, aged seventy-six.

NEWS and VIEWS

New Year Honours

The New Year Honours list includes the names of the following men of science and others associated with scientific and university work:

G.C.I.E.: Sir Maurice Gwyer, vice-chancellor of the University of Delhi.

G.B.E.: Sir Edward Mellanby, secretary of the Medical Research Council.

K.B.E.: Sir James Irvine, principal and vice-chancellor of the University of St. Andrews.

Knights: Prof. F. C. Bartlett, professor of experimental psychology, University of Cambridge; Mr. H. Claughton, recently principal of the University of London; Dr. J. D. Cockcroft, director of the Atomic Energy Research Establishment, Ministry of Supply; V. Z. de Ferranti, chairman and managing director of Ferranti, Ltd.; Dr. W. Ivor Jennings, vice-chancellor of the University of Ceylon; The Hon. C. J. Lowe, chancellor of the University of Melbourne; H. R. Ricardo, chairman and technical director, Ricardo and Co., Ltd.; Dr. R. V. Southwell, rector of the Imperial College of Science and Technology, London.

C.B.: Dr. G. M. Bennett, Government chemist.

C.S.I.: W. T. Hall, chief conservator of forests, United Provinces, India.

C.M.G.: A. P. Mitchell, director of land surveys, Palestine: W. A. Robertson, forestry adviser to the Colonial Office.

C.I.E.: J. Petty, chief conservator of forests, Sind; W. D. West, director of the Geological Survey of India; Dr. R. E. Mortimer Wheeler, director-general of archæology, India.

C.B.E.: H. W. Bennetts, principal of the Animal Health and Nutrition Laboratory, Western Australia; G. L. T. Brough, deputy director of electrical engineering, Admiralty; J. C. Bugher, director of the Yellow Fever Research Institute, Yaba, Lagos,

Nigeria; B. G. Crewe, assistant comptroller, Patent Office; W. J. Dawson, late metallurgical director of Messrs. Hadfields, Ltd.; W. S. Gordon, director of the Agricultural Research Council Field Station, Compton; J. Henderson, formerly director of the New Zealand Geological Survey; Mrs. S. Sutherland Isaacs, psychologist, London Clinic of Psycho-Analysis; Dr. L. A. Jordan, director of the Research Association of British Paint, Colour and Varnish Manufacturers; Prof. W. Kerr, Royal Technical College, Glasgow; F. Lambert, president of the Museums Association, director of the Walker Art Gallery, Liverpool; W. L. S. Mackintosh, director of Veterinary Services, Uganda; R. O'F. Oakley, assistant secretary, Department of Scientific and Industrial Research; Prof. G. W. Robinson, professor of agricultural chemistry, University College of North Wales, Bangor; Prof. C. Y. Shephard, professor of economics, Imperial College of Tropical Agriculture, Trinidad; G. F. Stebbing, honorary secretary of the Radium Commission; Miss M. V. Taylor, for services to archæology; J. S. Teasdale, for services to agriculture in Western Australia.

Prof. Hamilton Hartridge, F.R.S.

PROF. HAMILTON HARTRIDGE has recently retired from the University chair of physiology at St. Bartholomew's Hospital Medical College, London, in order to take up the post of director of the Medical Research Council's newly constituted research unit on the physiology of vision. The appointment shows the importance which the Medical Research Council attaches to this subject, and is a recognition of Prof. Hartridge's pre-eminent position and his contributions in this field. Educated at Harrow and King's College, Cambridge, Prof. Hartridge has had a distinguished academic and scientific career. A former fellow of King's College, he was lecturer on the organs of special sense and senior demonstrator in physiology at Cambridge at the time when Prof. Langley had surrounded himself with the brilliant team of investigators which included Keith Lucas, A. V. Hill and E. D. Adrian. Although Prof. Hartridge has published many important papers and articles on the physiology of the special senses, his research work has not been entirely confined to this field; the brilliant work which he carried out with Prof. F. J. W. Roughton on the rates of gaseous exchanges of the constituents of the blood is one of many examples of a wide interest in the problems of physiology. An account of Prof. Hartridge's activities would not be complete without reference being made to his great powers of exposition. He has delivered popular lectures at meetings of the British Association, given a course of Christmas Lectures at the Royal Institution and is Gresham professor of physic at Gresham's College in the City of London. After occupying a university chair for twenty years (1927–47), Prof. Hartridge carries with him the good wishes of a wide circle of friends for a continuation of his outstanding experimental work.

British Welding Research Association: Dr. H. G. Taylor

THE Council of the British Welding Research Association announces the appointment of Dr. H. G. Taylor as director in succession to Mr. A. Ramsey-Moon, who has been with the Association from its earliest stages and whose resignation has been announced. Dr. Taylor joined Messrs. Vickers, Ltd., at Barrow-in-Furness in 1921, and three years later obtained the firm's scholarship, with which he entered the University of London, graduating with first class honours in electrical engineering. He was awarded a Royal Scholarship and conducted research at the City and Guilds of London Institute, receiving the degree of M.Sc. (Eng.) for a thesis on the collection of current from commutators and slip rings. Joining Messrs. Metropolitan-Vickers Electrical Co., Ltd., as a college apprentice, he was later posted to the Research Department. In 1929 he joined the staff of the Electrical Research Association to start investigations on earthing, and many reports have since been published on this subject. With these Dr. Taylor was associated, and for them he received the degree of doctor of science in engineering of the University of London. In 1937, he was appointed electrical engineer to the Copper Development Association, and it was here that he commenced his connexion with the welding industry. This has been continued in his appointments as technical adviser to Philips Industrial Co., Ltd., and as manager of the Industrial Development Department of Philips Lamps, Ltd., in which posts he has been closely connected with the latest developments in arc and resistance welding.

Biochemistry in the University of California: Prof. W. M. Stanley

DR. WENDELL M. STANLEY, of the Rockefeller Institute for Medical Research, has been appointed professor of biochemistry in the University of California at Berkeley and director of a new Virus Laboratory which will undertake a programme of research on viruses affecting plants, animals and man. The new virus research centre is regarded as a major effort on the part of the University of California. Prof. Stanley, who is forty-three years old, was awarded a share of the Nobel Prize for Chemistry in 1946, for his work on the isolation, in pure form for the first time, of a virus, the tobacco mosaic virus (see *Nature*, **158**, 826; 1946).

Since this early work, which began in 1935, Prof. Stanley has remained in the forefront of research in his field. The isolation of viruses and the consequent determination of their physical properties opened the possibility of systematically producing chemical modifications of the disease agents for use as antigens in immunization. Prof. Stanley has already altered the chemical structure of the tobacco mosaic virus by means of known chemical reactions. He has also shown that mutation in viruses is accompanied by certain definite changes in chemical structure. During the War, Prof. Stanley and his colleagues developed an efficient method of producing a centrifuge-type influenza virus vaccine which was used in the Armed Services and is now available for civilian use.

U.S. National Academy of Sciences: Medal Awards

THE Daniel Giraud Elliot Medal for 1945 of the U.S. National Academy of Sciences has been presented to Prof. Sewall Wright, Ernest D. Burton distinguished service professor, University of Chicago, for his fundamental work dealing with the genetics of evolutionary processes, a programme based on work over a long period, including his paper "The Differential Equation of the Distribution of Gene Frequencies" (*Proc. Nat. Acad. Sci.*, **31**, No. 12; 1945). In making the presentation, Prof. Th. Dobzhansky said that in a series of papers published during the last twenty-five

years, and particularly since 1931, Prof. Wright has developed the modern mathematical theory of biological evolution. In the past, different authors attributed the principal role in evolution to different factors; there existed partisans of natural selection, of mutation, of hybridization, etc. Wright has shown that what is most important is the quantitative interrelations of all the factors. He has also discovered a new evolutionary agent, now known as the genetic drift or the Sewall Wright phenomenon. This hitherto almost unsuspected phenomenon is probably very effective, in connexion with the other previously known agents, in bringing about the theoretically most important and interesting form of evolutionary change, known as progressive evolution. The brilliant synthesis reached by Prof. Wright makes him the outstanding living evolutionist and the father of the modern neo-Darwinism.

The Public Welfare Medal for 1947 of the U.S. National Academy of Sciences has been presented to Karl Taylor Compton, president of the Massachusetts Institute of Technology, for his notable contributions of an original character to the science of physics, for his long and valuable career in the field of education and of university administration, and in recognition of his eminent service in the war-time research effort of the American nation, and in the reinforcing of collaboration and understanding between civilian scientific workers and military men. In his presentation address, Dr. A. R. Dochez pointed out that the Marcellus Hartley Fund was established and placed at the disposal of the National Academy of Sciences for the purpose of giving conspicuous recognition to eminence in the application of science to the public welfare. From the inception of the National Defense Research Committee, when the hazard of possible war emergency demanded a major effort by the United States, through the years of the emergency, to the final successful conclusion of hostilities, both as a physicist and as an administrator, Dr. Compton made outstanding contributions. As chief of the Office of Field Service of the Office of Scientific Research and Development, he headed a unique and eminently effective effort at partnership between military and civilian experts in bringing to bear the full strength of science and technology in the defence of the American way of life.

Research in Surveying at University College, London

THE institution in 1946 in the Department of Civil Engineering at University College, London, of the first chair in surveying and photogrammetry in the United Kingdom, to which Dr. C. A. Hart was appointed, has excited considerable interest in the Commonwealth and overseas. This has been particularly so because of recent advances in air survey and of the increasing employment of radar techniques both for air survey and geodetic surveying. At University College funds have been made available for the provision of first-class teaching and research equipment in surveying, with particular reference to photogrammetry. Emphasis on surveying in the Civil Engineering and Geography Schools has long been a feature at the College, and it is now proposed to institute a one-year special course of a postgraduate character in surveying, commencing in October 1948. This course will be arranged so as to provide a training ground for those who wish to enter the Colonial Survey Service or take up other careers of a similar character.

Colonial Geological Survey Service

AN important part of a comprehensive plan for the economic development of the British Colonies is a thorough investigation of their mineral resources. This involves a considerable expansion of the Colonial geological surveys and the completion of maps on reconnaissance and more detailed scales with the view to discovery of mineral deposits and the determination of their economic significance. Such a programme of investigation offers geologists a wide range of interest—scientific and economic—with the added attraction of pursuing field-work under varied geographical and climatic conditions. There is place, too, for those who wish to specialize in petrology, mineralogy, economic geology, geophysics, etc. Most Colonial geological surveys offer opportunities for research, and men are encouraged to do new work and prepare for research degrees. Wherever possible they are given facilities also to study special subjects related to their work. A considerable number of geologists will be required during the next four years and a slightly lesser number in succeeding years to carry out such a programme. The vacancies in view are announced so far as possible in advance so that men who desire an active life of field-work may be encouraged to pursue geological studies with reasonable prospect of securing an appointment in the Colonial Geological Survey Service. The vacancies anticipated during the next few years are as follows: 1948–52, an average of 25 a year; 1953–56, an average of 15 a year. The Colonies in which these vacancies will occur include the following: Kenya, Uganda, Tanganyika Territory, Northern Rhodesia, Nyasaland, Bechuanaland, Swaziland, Nigeria, Gold Coast, Sierra Leone, British Guiana, British Honduras, Malaya, North Borneo, Sarawak, and certain other smaller Colonies. The posts offered are, in general, permanent and pensionable in themselves, or such as to lead to permanent and pensionable posts; temporary service would ordinarily count for pension where there has been no break in service. The qualification required is a good honours degree in geology, preferably supplemented by a research degree or research experience. Forms of application for appointment to the Colonial Geological Survey Service are obtainable on request from the Director of Recruitment (Colonial Service), Colonial Office, 15 Victoria Street, London, S.W.1.

A Standard for Quality Control

AN increasing number of British Standards refer to quality control in acceptance clauses, and B.S. 1313: 1947 has been prepared to meet the need for a publication which would describe the simplest method of quality control, by the examination of the fraction defective in samples. It sets out accepted methods which are easy to use and which may safely and profitably replace 100 per cent routine examinations. They are widely applicable, and the publication, without dealing with the statistical principles underlying the methods, attempts to show how to use them. Simple instructions are given for the construction and use of a quality control chart in the first section. The second section deals with the methods in more detail, describing the basis of fraction-defective charts, with detailed recommendations for sample size and interval, and normal control limits. Refinements in the general method are indicated in the third section, such as the uses of average level of defectives, two-way control charts

and compressed-limit control charts. The necessary tables with instruction sheets containing fully worked out examples are given in a further section, and a bibliography is appended.

Air-depolarized Primary Cells

BRITISH STANDARD SPECIFICATION No. 1335 ; 1946 (Publications Sales Department, British Standards Institution, 28 Victoria Street, London, S.W.1. 2*s.* post paid) prescribes the minimum rating of air-depolarized-type primary cells on continuous discharge and gives the methods of test on which the rating is based. An air-depolarized cell is defined as a primary cell with two electrodes, one mainly of carbon and the other of zinc, in an electrolyte of ammonium chloride, or other salts, or caustic soda, and in which the depolarization is mainly effected by air, to which free access must be given. A nomenclature is prescribed for the designation of the cells, a letter representing the type, and a number the size of cell. The letter code to be used consists of : *A*, *D*, *W*, *R*, *S*, and *C*, for air-depolarized, dry, wet, round, square and caustic soda, cells respectively. The quality of the materials to be used, the dimensions of the electrodes, containers, connecting wires and terminals of the cells are also specified in detail.

Rubber Technology Conference

IN 1938 the Institution of the Rubber Industry organised an International Rubber Technology Conference in London. It was the intention of the Institution to organise another conference within the following two or three years ; but it has been necessary to wait ten years. A conference will accordingly be held at the Central Hall, Westminster, London, S.W.1, during June 23–25. The Conference will cover the following subjects : natural and synthetic rubber lattices ; chemistry of rubber ; physics of rubber ; testing and analysis ; synthetic rubbers ; compounding ingredients ; fibres and textiles ; developments in factory processes and products since 1938. Invitations to attend the Conference have been sent to well-known technologists all over the world, many of whom have already promised papers. All the papers and full details of the Conference will be published in a volume entitled "The Proceedings of the Rubber Technology Conference, London, 1948", a copy of which will be issued free to all those enrolling for the Conference ; extra copies will be available for sale generally at £2 2*s.* each. Application forms and details can be obtained from the Conference Secretary, Institution of the Rubber Industry, 12 Whitehall, London, S.W.1, who is prepared to assist in obtaining hotel accommodation.

University of London : Appointments

THE following appointments in the University of London have been announced :

Mr. G. E. H. Foxon, lecturer in zoology and comparative anatomy at University College, Cardiff, to the University readership in biology tenable at Guy's Hospital Medical School as from April 1.

The following titles have been conferred : professor of bacteriology in the University on Lord Stamp, in respect of the post held by him at the Postgraduate Medical School of London ; professor of biology in the University on Dr. J. H. Woodger, in respect of the post held by him at Middlesex Hospital Medical School ; reader in surveying in the University on Mr. Alfred Stephenson, in respect of the post held by him at the Imperial College of Science and Technology.

The degree of doctor of science has been conferred on Mr. F. W. Andrews (Birkbeck College).

Colonial Service : Appointments

THE following appointments have been made in the Colonial Service : P. A. Greig, agricultural development officer, Nigeria ; W. G. Brookbank, agricultural officer, Tanganyika ; D. H. Drennan, agricultural officer, Tanganyika ; N. B. Flockhart, agricultural officer, North Borneo ; R. Henderson, agricultural officer, Malaya ; W. J. C. Little, agricultural officer, Nigeria ; R. R. Mason, agricultural officer, Fiji ; D. G. Thomas, botanist, Uganda ; I. H. Warren, agricultural chemist, Tanganyika ; J. R. Williams, entomologist, Mauritius ; D. W. Winter, agricultural officer, Zanzibar ; R. S. Ayliffe, assistant conservator of forests, Trinidad ; J. A. D. Jackson, assistant conservator of forests, Nigeria ; J. A. Laurenson, assistant conservator of forests, Sierra Leone/Gambia ; F. J. S. Phillips, assistant conservator of forests, Nigeria ; A. W. M. Watt, assistant conservator of forests, Uganda ; D. A. J. Wood, assistant conservator of forests, Gold Coast ; J. R. Harpum, geologist, Tanganyika ; H. R. Threlfall, surveyor, Tanganyika ; D. L. Johns, research officer (entomologist), Tanganyika ; A. F. MacKenzie (agricultural officer, Sierra Leone), senior agricultural officer, Sierra Leone ; J. F. Ward (agricultural officer, Kenya), senior agricultural officer, Nigeria ; F. A. Cassidy (inspector of mines, Sierra Leone), senior inspector of mines, Nigeria.

Announcements

THE Council of the Linnean Society of London has nominated the following as recipients of the Trail Award and Medal : for 1942, Dr. Honor B. Fell, of the Strangeways Laboratory, Cambridge ; for 1947, Dr. C. D. Darlington, director of John Innes Horticultural Institution. The Trail Award and Medal was established by a donation by the late Prof. J. W. H. Trail "to encourage study that throws light on the substance known as Protoplasm, or the physical basis of life" and is awarded by the Council at intervals of not less than five years. The presentations of the Award and Medal will be made at the Society's anniversary meeting on May 24.

H. B. HENBEST has been elected to a Beit Fellowship for Scientific Research of the value of £400 a year, tenable at the Imperial College of Science and Technology, London, for research in organic chemistry under the direction of Sir Ian Heilbron.

THE late Dr. Collinge's collection of terrestrial Isopoda has been presented to the Yorkshire Museum, to which institution all future inquiries with regard to the collection should be addressed. The collection is important since it contains a large number of Collinge's described species and varieties. A list will be supplied on loan to any interested worker on application to the Keeper, Yorkshire Museum, York.

ERRATUM.—Referring to the communication on the 'floating thread' in *Nature* of November 15, 1947, p. 673, I. Kuščer writes : "The formula at the bottom of the left-hand column should be $d = \pi(\sigma_0 - \sigma_1)^2 \div 4\mu\rho v_0^2$. In both expressions for y (right-hand column) substitute 1 for λ. The unit 'cm.' against the value for the derivative $(dy/dx)_0$, which is obviously dimensionless, should be omitted."

LETTERS TO THE EDITORS

The Editors do not hold themselves responsible for opinions expressed by their correspondents. No notice is taken of anonymous communications

Variation of Geomagnetic Intensity with Depth

In a recent letter[1], A. L. Hales and D. I. Gough compared their measurements of H_d, the horizontal magnetic intensity (H) at depth d, made in the Witwatersrand at $d = 4{,}800$ ft., with the formula

$$H_d/H_0 = 1 - 2\,(5\rho_1/\rho + 1)\,d/a \qquad (1)$$

obtained by S. K. Runcorn from Blackett's theory[2]. In (1), H_0 denotes the value of H at the overlying point on the surface ($d = 0$), a denotes the earth's radius, ρ_1 the mean density of the rocks down to depth d, and ρ the overall mean density of the earth. The observations were compared also with the formula

$$H_d/H_0 = a^3/(a-d)^3 = 1+3d/a \text{ (approximately)}, \qquad (2)$$

corresponding to any 'core' theory which places the source of the field wholly below the depth d.

It is clear that (1) cannot be correct, because it should agree with (2) if $\rho_1 = 0$, whereas in fact it gives $H_d/H_0 = 1 - 2d/a$ in that case. The correct formula, which can be derived from a vector potential or otherwise, is

$$H_d/H_0 = 1 - 3(5\rho_1/k\rho - 1)d/a, \qquad (3)$$

where k denotes I/I_0, and I signifies the moment of inertia of the earth and I_0 that of a uniformly dense sphere of the same size and mass. The proof of this and allied and more general formulæ will be communicated to the Physical Society.

Taking $\rho = 5{\cdot}5$, $\rho_1 = 2{\cdot}8$, $k = 0{\cdot}88$, the factor of d/a in (1) is $-7{\cdot}1$, and in (3) is $-5{\cdot}7$. Hence $\Delta H \equiv (H_d - H_0)$, which for comparison with the Witwatersrand observations was calculated[1] as $-26\,\gamma$ from (1), should be $-21\,\gamma$ according to (3), whereas (2) gives $+11\,\gamma$. The mean observed value, after making various alternative possible corrections for the effects of local magnetic matter, was given as lying between -10 ± 4 and $-20 \pm 4\,\gamma$. These are not in accord with (2); but to what extent ΔH agrees with (3) must remain uncertain until a more unambiguous determination is obtained.

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[1] Hales, A. L., and Gough, D. I., *Nature*, **160**, 746 (1947).
[2] Blackett, P. M. S., *Nature*, **159**, 658 (1947).

The formula giving the variation of the earth's horizontal field with depth quoted in the letter of Hales and Gough involves an approximation concerning densities which is only applicable to spheres the density distribution of which is only slightly different from a uniform one, such as is roughly true for the earth. Thus it is not to be expected that the formula should give the dipole variation law when $\rho_1 = 0$, but I am grateful to Prof. Chapman for pointing out a more general formula. It should be noted, however, that in the present state of the theory of Blackett's formula, $P = \beta \dfrac{G^{1/2}}{2c} U$, the treatment of densities must remain ambiguous, various assumptions leading to slightly different values of the predicted variation with depth. This point will be dealt with in detail elsewhere.

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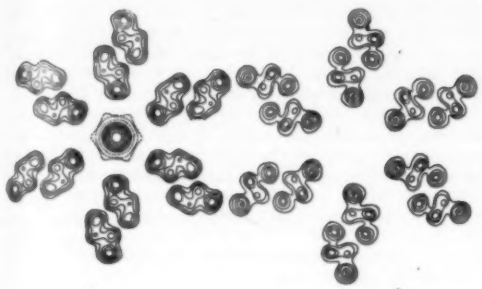
Beta-quinol: an Example of the Firm Union of Molecules Without the Formation of Chemical Bonds Between Them

The substance formerly called 'β-quinol' and obtained by crystallization of quinol from methanol has been shown[1] to be a compound of three molecules of quinol with one molecule of methanol. It is one of a series of solid compounds of very similar crystalline structure[2], which consists of two interpenetrating giant molecules of hydrogen-bonded quinol units together enclosing small molecules, such as that of methanol, in cavities that are left when these two giant molecules are fitted together as closely as possible. Compounds of this general type in which one component imprisons the other without necessarily having any strong attractive interaction with it have been named clathrate compounds[3]. When crystallized, for example, from ethanol in conditions such that molecules of suitable size are not available for enclosure in this way, quinol forms a more densely packed structure, α-quinol, by a different arrangement of quinol units which has not yet been completely determined.

In the clathrate compounds of quinol the presence of the enclosed molecules exerts a stabilizing effect on the double cagework quinol structure which, although it represents a simple way of satisfying the directional requirements of the hydrogen bonds linking the quinol units, is clearly of a very open character since it provides space for the second component. However, since some of the compounds are found to exist with a large proportion of the cavities unoccupied[4], it seemed possible that the completely empty structure might be capable of existence. Single crystals of quinol have now been obtained in this form. This crystalline form is given the name 'β-quinol', which should no longer be used for the methanol compound. It differs in unit-cell dimensions, refractive indices and density from α-quinol, and a preliminary Fourier electron-density synthesis shows that the structure is very similar to those already determined for the clathrate compounds 3 quinol.M', where M is SO_2, MeOH or a variety of other small molecules, with the essential difference that the spaces occupied in the compounds by molecules of the second component are here vacant. This may be seen in a comparison of the accompanying electron density maps for (*a*) 3 quinol.SO_2, (*b*) the empty β-quinol structure. Both show the projected electron density on the (0001) face of the crystal. Whereas the compound shows in the centre a high circular peak, corresponding to the (rotating) sulphur dioxide molecule, and the six surrounding quinol molecules which form its cage, the β-quinol projection shows similar quinol molecules with an empty space at the centre. In both projections the benzene rings are considerably out of the plane of the projection and consequently they appear distorted from their true regular hexagonal shape.

It may be shown from the unit-cell dimensions and from the number and dimensions of the cavities contained within it that in β-quinol about $7\frac{1}{2}$ per cent of the total volume is empty space that could be



a *b*

occupied by other molecules, and is so occupied in the clathrate compounds. In any structure regarded as composed of spherical atoms of fixed radius, a part of the total space is represented by the unoccupied small cavities between spheres in contact, and even in a closest packed arrangement of equal spheres this is a considerable proportion. The 7½ per cent mentioned above is in addition to the space of this sort.

β-Quinol is the first example of a special type of molecular compound. It is composed of two giant molecules which, although not linked by bonds of any sort, are held together in a manner much firmer than corresponds to van der Waals' interaction. Furthermore, the two molecules cannot be separated without the breaking of bonds, for example, hydrogen bonds, although these bonds do not connect the two molecules.

The state of the molecules may be described as follows. Let the skeleton of a steel-frame building represent a portion of the indefinitely extended three-dimensional giant molecule. Let a second identical framework be constructed displaced relative to the first so that each junction of girders in one framework is at the space centre of a compartment of the other framework. There is no join between the two systems of girders, and there is therefore some freedom of independent movement of the two; but they cannot be separated since each makes a multiple enclosure of the other and thus imprisons it. If the girders are regarded as quinol molecules and the junctions between them are taken to be hydrogen bonds, the description applies in all essentials to β-quinol.

The imprisonment of one molecule by the other means that β-quinol is to be regarded as a special type of clathrate compound. Although a similar arrangement of molecules occurs in the compounds mentioned above, the complete absence of any enclosed molecules removes all possibility that they might play any part in the union of the two giant molecules. It is thus demonstrated that two molecules, which in this case happen to be identical in composition and structure, may be united as firmly as though there were hydrogen bonds between them, although, in fact, no such bonds exist.

Details of the crystal structure of β-quinol will be published elsewhere.

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[1] Palin, D. E., and Powell, H. M., *Nature*, **156**, 334 (1945).
[2] Palin, D. E., and Powell, H. M., *J. Chem. Soc.*, 208 (1947).
[3] Powell, H. M., *J. Chem. Soc.* (in the press).
[4] Palin, D. E., and Powell, H. M., *J. Chem. Soc.* (in the press).

Use of Ultra-Violet Fluorescence in Paper Chromatography

It has been shown[1] that, by the action of chymotrypsin, insulin is broken down into a number of peptide fragments of comparatively small molecular weight, and a more massive residue. The higher molecular weight material can be precipitated by means of trichloro-acetic acid. Attempts have been made to separate the lighter peptide mixture, and the most effective method so far found has been paper partition chromatography[2].

With phenol as the mobile phase, the mixture is resolved into four broad bands each of which can be further resolved by the use of collidine. It has been found during these experiments that the α-amino-acids and peptides can be detected on the filter paper sheets by drying off the solvent and examining the sheet under illumination from a 'Hanovia' lamp in a darkroom, using a ⅜ in. thick Wood's glass filter.

De Ment has noted[3] the fluorescence of the common α-amino-acids and a few dipeptides in the solid state when illuminated with light of 3650 A. wave-length. On paper chromatograms this fluorescence can be seen even though drying has only been done at 37° C., but the intensity increases markedly when higher drying temperatures are used. The quality of the fluorescent light is visually about the same for all the acids and peptides, a whitish-violet colour standing out against the dark violet of the filter paper. Using this method, it is possible to mark out the positions of the substances and cut out the spots or bands for elution, without sacrificing material for ninhydrin testing. Sheets marked in this way and afterwards tested with ninhydrin have shown the coincidence of the markings and the colours; but the ninhydrin test is the more sensitive one, since 20 μgm. of an amino-acid per square inch of paper is about the least visible under the ultra-violet light used. The chief drawback of the method is that extraneous fluorescent substances can be obtained from the paper and solvents used. These impurities may not produce a chromatograph pattern until they have been eluted from a sheet and run with a different solvent on a fresh chromatogram.

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[1] Butler, J. A. V., Dodds, E. C., Phillips, D. M. P., and Stephen, J. M. L., *Biochem. J.* (in the press).
[2] Consden, R., Gordon, A. H., and Martin, A. J. P., *Biochem. J.*, **38**, 224 (1944).
[3] De Ment, J., "Fluorescent Chemicals and their Applications" (New York, 1942).

Inhibition of the Photodynamic Action of 3 : 4 Benzpyrene

It has gradually become apparent that the carcinogenic activity of 3 : 4 benzpyrene is linked with sulphur metabolism[1]. Some experiments have, therefore, been carried out to attempt to determine whether another biological property of 3 : 4 benzpyrene, namely, its photodynamic action, is involved with sulphur. These were done by measuring the photodynamic activity of the benzpyrene in the presence of sulphur containing compounds.

For this purpose thick cultures of Paramecia were used as a biological indicator. Irradiation was from a mercury-vapour lamp screened with a glass filter opaque to radiations of less than 3,300 A. Exposures

were made using large well-slides to contain Paramecia and the media. The benzpyrene was used in the form of a colloid in tap water.

Preliminary experiments were carried out to determine the toxicity and any photodynamic action of the compounds under test. Toxic concentrations were taken as those which would cause cytolysis within 30 min.; tolerance concentrations as those which had no apparent effect after two hours exposure. The results of these trials are given in Table 1.

TABLE 1

Compound	Toxic conc.	Tolerance conc.	Photodynamic action at tolerance conc.
Cysteine hydrochloride	1/1250	1/1500	Nil
Glutathione	1/1250	1/2000	Nil
Methionine	1/225	1/250	Nil

Using concentrations which were non-toxic, these substances were tested again in the presence of various concentrations of 3:4 benzpyrene. Controls, in which the test substance was replaced by an equivalent volume of tap water, were irradiated side by side with the experimental cultures. In all cases the test substance and the benzpyrene colloid were added to the Paramecia cultures immediately before commencement of irradiation.

Of the compounds mentioned in Table 1, the only one causing any variation in the time required for cytolysis of 90 per cent of the Paramecia as compared with the time required for controls was cysteine hydrochloride. The results obtained using 0·03 per cent cysteine hydrochloride and varying concentrations of benzpyrene are shown in Fig. 1.

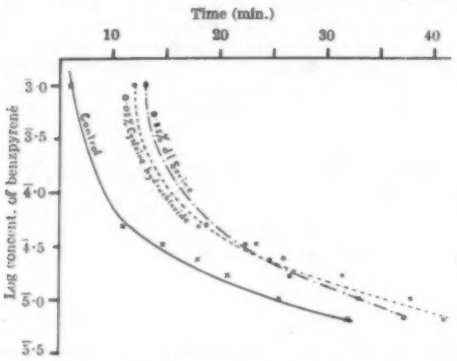

FIG. 1. INHIBITION OF PHOTODYNAMIC ACTION OF BENZPYRENE IN THE PRESENCE OF CYSTEINE HYDROCHLORIDE AND SERINE

Further tests were carried out with a series of amino-acids to determine whether any similar inhibition of photodynamic action could be obtained. The manner and conditions of testing were the same as used previously. The toxic concentrations, tolerance concentrations and photodynamic effects are shown in Table 2.

TABLE 2

Compound	Toxic conc.	Tolerance conc.	Photodynamic action at tolerance conc.
Glycine	1/225	1/250	Very slight
Histidine	1/250	1/300	Nil
Phenylalanine	1/200	1/250	Nil
d-l-Serine	1/200	1/250	Nil
Threonine	1/250	1/300	Very slight
d-l-Valine	1/200	1/250	Nil

Of this group of compounds, the only one having any effect on the photodynamic action of the benzpyrene was *d-l*-serine. The effect of using a 0·25 per cent solution is an inhibition very similar to that given by the cysteine hydrochloride (see Fig. 1).

Further results were obtained using a fixed concentration (0·001 per cent) of the benzpyrene and varying strengths of the cysteine hydrochloride and *d-l*-serine. The results are shown in Fig. 2.

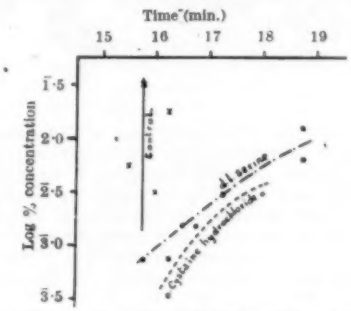

FIG. 2. VARIATION IN DEGREE OF INHIBITION WITH VARIATION IN CONCENTRATION OF INHIBITORS

The significance of these inhibitory actions and the manner in which they are exercised are still unknown, though it is interesting to recall the close chemical relationship between cysteine and serine. Further experiments are in hand to try to determine details of these actions.

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[1] Crabtree, H. G., *Brit. Med. Bull.*, 4 (5 and 6), 345 (1947).

Reduction of 1 : 4-Diketones

THE dibenzoylethylenes on reduction with metal combinations or catalytic hydrogenation under certain conditions yield the corresponding diphenacyls[1], and there is evidence that the reaction occurs through 1 : 6-addition of hydrogen followed by rapid ketonization of the intermediate dienols[1]. Lutz and Reveley[2] showed the presence of one such dienol, without, however, isolating it:

$$R.C(O).CH:CH.C(O).R \rightarrow [R.C(OH):CH.CH:C(OH).R] \rightarrow R.CO.CH_2.CH_2.CO.R.$$

Confirmation of this mechanism is desirable, especially since 1 : 6-addition is comparatively uncommon[3]. We have done this by reducing several substituted dibenzoylethylenes with aluminium *iso*propoxide in *iso*propyl alcohol, a reagent known to attack carbonyl groups while leaving olefinic linkages intact. The reduction products were invariably the corresponding diphenacyls, identified (after chromatographic adsorption) by mixed melting points with authentic samples. Since direct hydrogenation of the carbon–carbon double bonds is ruled out, the reduction must have taken the course outlined above.

	Reduction product and yield	per cent
trans-Dibenzoylethylene	Diphenacyl	70
cis-Dibenzoylethylene	Diphenacyl	60
trans-Di-*p*-toluylethylene	4 : 4'-Dimethyldiphenacyl	75
cis-Di-*p*-toluylethylene	4 : 4'-Dimethyldiphenacyl	95
trans-Di(*p*-chlorobenzoyl)-ethylene	4 : 4'-Dichlorodiphenacyl	80
cis-Di(*p*-chlorobenzoyl)-ethylene	4 : 4'-Dichlorodiphenacyl	90
trans-Di(*p*-bromobenzoyl)-ethylene	4 : 4'-Dibromodiphenacyl	75
cis-Di(*p*-bromobenzoyl)-ethylene	4 : 4'-Dibromodiphenacyl	90

It is noteworthy that the *cis*-compounds react more rapidly with this reducing agent. With the *trans*-isomers some unchanged substance was always found in the product.

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[1] Conant, J. B., and Lutz, R. E., *J. Amer. Chem. Soc.*, **45**, 1047 (1923); Lutz, R. E., *ibid.*, **51**, 3008 (1929); Lutz, R. E., and Palmer, F. S., *ibid.*, **57**, 1947, 1953, 1957 (1935).
[2] Lutz, R. E., and Reveley, W. G., *J. Amer. Chem. Soc.*, **61**, 1854 (1939).
[3] Allen, C. F. H., and Blatt, A. H., Gilman's "Organic Chemistry", 1, 603 (1938).

Total Conjugation of Linoleic Acid in Oxidation with Lipoxidase

Bolland and Koch[1] postulated the mechanism of autoxidation of linoleate to be:

$$-CH_2.CH : CH.CH_2.CH : CH.CH_2- \quad \text{(I)} \xrightarrow{-H^\bullet}$$
(13) (12) (11) (10) (9)

$$\left[\begin{array}{ll} -CH_2.CH : CH.\overset{\bullet}{C}H.CH : CH.CH_2- & \text{(II)} \\ -CH_2.CH : CH.CH : \overset{\bullet}{C}H.CH_2- & \text{(III)} \\ -CH_2.\overset{\bullet}{C}H.CH : CH.CH : CH.CH_2- & \text{(IV)} \end{array}\right] \xrightarrow[+H^\bullet]{+O_2}$$

$$-CH_2.CH : CH.CH.CH : CH.CH_2- \quad \text{(V)}$$
$$\qquad\qquad\quad |$$
$$\qquad\qquad OOH$$

$$-CH_2.CH : CH.CH : CH.CH.CH_2- \quad \text{(VI)}$$
$$\qquad\qquad\qquad\quad |$$
$$\qquad\qquad\qquad OOH$$

$$-CH_2.CH.CH : CH.CH : CH.CH_2- \quad \text{(VII)}$$
$$\quad\; |$$
$$\quad OOH$$

From the ultra-violet absorption due to the conjugation of the double bonds and from the active hydrogen content of the resulting mixture of peroxidic products, they concluded that it consisted of approximately equal parts of the hydroperoxides V, VI and VII, indicating that the free radical structures II, III and IV contribute equally to the free radical resonance hybrid.

Other workers' results are in agreement with the general outlines of the mechanism[2,3]. In the hydrogenated products, Bergström[3] identified comparable amounts of 9- and 13-hydroxystearate, but was unable to detect the 11-isomer.

The oxidation of linoleates in aqueous solution with crude soya-bean lipoxidase seems to follow the same general course as the autoxidation[2,3]. It was observed that the extinction at 234 mμ per absorbed oxygen increased when the enzyme was purified[4].

When crystalline lipoxidase[5] became available, the problem was reinvestigated under conditions least likely to promote side reactions or decompositions. The oxidations were carried out in Warburg vessels under air, using sodium linoleate substrate[4]. The reactions were stopped by addition of alcohol, and light absorption was measured using the Beckman spectrophotometer.

Apparent molar extinction coefficients of the primary oxidation product at 234 mμ were found to be 23,000 at 37°, 27,800 at 8° and 31,400 at 0°, indicating that reaction temperature affects the formation or decomposition of conjugated diene to a marked degree. Reduction of the extinction by elevated temperature is accompanied by increased absorption at 270 mμ. The extinction coefficient at 234 mμ remains near 30,000 up to 30 per cent oxidation at 0°, whereas at 37° it has decreased to 20,000 at 10 per cent oxidation.

Thus in the rapid enzymatic oxidation of linoleate at low temperature, one pair of conjugated double-bonds is formed for every oxygen absorbed. This indicates that III and IV are the main contributing structures to the free radical resonance hybrid, resulting in oxygen addition at the ends of the resonating system, giving only the conjugated hydroperoxides VI and VII. Variable amounts of products, other than conjugated hydroperoxides, formed at higher temperatures, with longer reaction times, or with impure enzyme, appear to be attributable to decompositions or secondary oxidations of conjugated peroxides.

Our evidence suggests that a similar mechanism may account for the low extinction coefficients observed in the autoxidation of linoleates.

A full report will appear in due course (*Arch. Biochem.*).

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[1] Bolland and Koch, *J. Chem. Soc.*, 445 (1945).
[2] Bergström, *Nature*, **156**, 717 (1945); *Arkiv Kemi, Min.-Geol.*, **21**, A, 14, 15 (1945).
[3] Holman, *Arch. Biochem.*, **10**, 519 (1946).
[4] Theorell, Bergström and Åkesson, *Pharm. Acta Helv.*, **21**, 318 (1946).
[5] Theorell, Holman and Åkesson, *Arch. Biochem.*, **14**, 250 (1947).

Ammonium Salts of Sulphanilamides and Sulphonic Acids

SULPHANILAMIDE salts of a few sulphonic and carboxylic acids have been described[1,2,3], but they have not been isolated in pure form and characterized. In the course of our work on surface-active compounds, and with the object of examining their activity as intestinal bacteriostatic agents, we have prepared sulphanilamide salts of di*iso*propyl naphthalene sodium sulphonate (m.p. 220–22°), sodium dioctylsulphosuccinate, and lauryl-*p*-toluidide-2-sodium sulphonate. Suitable experimental conditions were first determined by the preparation of (*a*) salts of naphthalene-β-sulphonic acid and sulphanilamide (m.p. 263–64°), sulphathiazole (214–15°), sulphapyridine (152–53°), sulphadiazine (188°), sulphamerazine (118–120°), sulphaguanidine (225–26°), N^1-acetylsulphanilamide (254–55°), and *p*-aminobenzoic acid (decomp. above 260°); and (*b*) sulphanilamide salt of phenol-*p*-sulphonic acid (241–42° decomp.). The last salt, described earlier[1] as giving m.p. 216–220° (decomp.), was apparently a mixture containing sulphanilamide hydrochloride.

The bacteriostatic activity (mgm. per cent) of salts of sulphanilamides and naphthalene-β-sulphonic acid is given in the table:

Sulpha drug	Sulpha drug in salt (%)	*St. aureus* Salt	*St. aureus* Parent drug	*Escherichia coli* Salt	*Escherichia coli* Parent drug
Sulphanilamide	45·0	500	500	500	500
N^1-Acetylsulphanilamide	50·7	150	200	150	225
Sulphapyridine	54·4	12	22	14	22
Sulphathiazole	55·1	8	12	8	10
Sulphadiazine	54·3	8	20	10	22
Sulphamerazine	56·1	20	40	30	30
Sulphaguanidine	50·7	250–300	250	250	300

The results indicate that the ammonium salts were more active than the parent sulphanilamides in spite of the lower content of the active sulpha component. The increased potency of our salts could not be due to increased solubility[2], as they were not more soluble than the sulpha components. The potentiation of the sulphanilamides by ammonium salt formation could be due to ionization, resulting in the sulpha component acquiring the highly active zwitterion form $^{+}\left[H_3N-\bigcirc-SO_2NR\right]^{-}$, stabilized by the complementary inductive effects of the positively charged ammonium and the negatively charged sulphonamide groups. Inactivation of the sulpha drugs *in vivo* during metabolism by N^4-acetylation might perhaps be prevented or minimized by the use of sulphanilamide salts instead of the parent sulpha component.

Further, we have found that replacement of sulphanilamide in its salts with naphthalene-β-sulphonic acid by *p*-aminobenzoic acid took place to a small extent even at room temperature (26–29°), when an aqueous solution of *p*-aminobenzoic acid and the sulphanilamide salt was kept for ten days. The sulphonic anion might, therefore, make a small contribution to the bacteriostasis by removing part of the essential metabolite, *p*-aminobenzoic acid, by salt formation, as also by complex formation with the positively charged part of the enzyme molecule.

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[1] U.S.P. 2,182,075.
[2] Smyth and Carpenter, *Science*, **87**, 350 (1938).
[3] B.P. 490,350. F.P. 831,300. Mossini, *Boll. Chim. Farm.*, **78**, 429 (1939).

Effect of Under-nutrition and Alterations in Diet on the Choline Esterase Activity of Serum

The activity or 'concentration' of choline esterase in serum has been considered as a test of liver function[1], and an interest in this enzyme has been stimulated in recent years by the developments which took place in chemical warfare during the period of hostilities. So far, diet has not been implicated as one of the agents regulating the activity of either the 'total' or the 'true' choline esterase of serum; but within the last eighteen months we have made numerous observations which show that the general level of nutrition and also certain changes of diet may exert a considerable influence. All the determinations of activity were made by measuring the carbon dioxide evolved by the enzyme in a Barcroft apparatus. The enzyme activity of a serum such that 1 c.c. liberates 1 c.mm. of carbon dioxide in 1 min. from the reaction mixture has been taken as unity. The 'total' choline esterase in serum was measured in the presence of 2·5 per cent of acetyl choline chloride. This concentration of substrate is well above the optimum for true choline esterase, and the activity determined by this method is due mainly to 'pseudo' choline esterase. The 'true' choline esterase in the serum was determined by the method of Mendel, Mundell and Rudney[2].

An average of 80·2 units of 'total' choline esterase (*S.D.* 15·5) was found in the sera of thirty-nine healthy British men who were living on British civilian or army rations. The mean concentration in a group of eighty-two German factory workers living on German civilian rations in 1946 was found to be 56·7 (*S.D.* 14·5), and forty-five German civilians, who were attending a hospital in Wuppertal-Barmen on account of nutritional œdema, gave an average of 48·5 (*S.D.* 16·2). An average of 42 units was obtained from twelve German civil internees who had œdema, and the very low average of 29·0 units from a group of nine civilian prisoners in a gaol at Kiel. Although severe under-nutrition and œdema may be accompanied by low serum proteins, there is good evidence that the changes in choline esterase were not due to this.

Effect of an improved diet on the choline esterase activity of serum

	'Total' choline esterase activity (c.mm. CO_2/c.c./min.) Average and range	Standard deviation	'True' choline esterase activity (c.mm. CO_2/c.c./min.) Average and range	Standard deviation
12 civilian prisoners, July 1946	51·56 (27·6–88·2)	18·60	0·807 (0·596–1·130)	0·143
The *same* 12 prisoners after 5 months on improved rations	62·90 (32·4–96·4)	18·00	1·050 (0·749–1·270)	0·127
19 under-nourished German civilians, September 1946	47·40 (29·8–63·5)	7·65	0·917 (0·710–1·140)	0·127
The *same* 19 civilians after 2 months high calorie diet, November 1946	75·20 (59·4–101·5)	13·00	1·020 (0·789–1·370)	0·145
The *same* 19 civilians after 5 months on German rations, April 1947	55·40 (39·3–82·2)	11·49	0·969 (0·775–1·330)	0·145

A rise in the choline esterase has twice been observed to follow an improvement in the diet (see table). Between July 1946 and December of the same year, twelve civilians in Siegburg prison gained an average of 10 kgm. in weight because they were given more food. The effect of this on the choline esterases in their sera will be seen in the table. The differences were statistically significant both for 'total' and for 'true' choline esterase ($t = 2·56$, $p = 0·05–0·02$ and $t = 4·43$, $p < 0·01$ respectively).

In the autumn of 1946, nineteen under-nourished German adults were given for experimental reasons an excellent diet, unlimited in quantity, for eight weeks. This produced an average rise in the body-weight of 10 kgm. and an increase in their mean 'total' choline esterase which was highly significant ($t = 12·2$, $p < 0·01$). The increase in 'true' choline esterase activity was also statistically significant ($t = 2·63$, $p = 0·02–0·01$).

When these men returned to their civilian rations in November 1946, most of them lost weight again, and their choline esterases fell also during the next five months. The decrease in total choline esterase was statistically significant ($t = 6·6$, $p < 0·01$). The diets were not controlled or supervised during these

five months, and three of the men who managed to maintain their weights better than the others also maintained their serum choline esterases much better. It is not thought that these increases in choline esterase were due to the digestion, absorption and metabolism of fat setting free non-specific esterases into the serum, for when six men were given a supplement of 100 gm. of fat daily for three days there was no increase in the 'total' choline esterase found in their sera.

These results, and others which have not yet been published, suggest that the average level of the 'pseudo' enzyme in the serum is a delicate index of the state of nutrition of any group of men in Germany to-day.

It is hoped to publish the results of this work in full in the near future, and various aspects of it are being actively studied at the moment; but we think it may be helpful to those working on the enzymes to know at this stage that we have obtained these results. The knowledge may possibly assist them to establish the relationship of the enzymes to the liver and to the other organs involved in their functional activity.

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Premortal Increase in the Output of Sodium and Chlorine in Fasting Rats

Kunstmann, in an experiment on himself[1], drank 10–20 litres of water daily, with an otherwise normal food intake. He felt fit during the experiment, his urinary output in sodium and chlorine sank to minimal values; but nevertheless his sodium chloride balance became negative. On about the thirtieth day of the experiment, his chlorine excretion rose to abnormally high values and he felt ill, so he ended the experiment. We attempted to reproduce this experiment with rats. We gave them maize washed out in water, and twice daily 10 ml. of a 5·5 per cent glucose solution subcutaneously. In this way we attained great quantities of a rather dilute urine; the animals slowly lost weight; but the output of sodium and chlorine showed no increase, even on the fiftieth day of the experiment, and we failed to obtain a negative salt balance.

We decided, therefore, to make the rats fast, without or with water *ad libitum*, with twice daily 10 ml. of a 5·5 per cent glucose solution subcutaneously; we collected urine specimens daily and analysed them for sodium, potassium, chlorine and nitrogen. In all cases, on the last day before death, the output of sodium and chlorine was three to six times as great as on the previous days. We varied our experiments by giving the rats free access to water, but no extra injections. In about half the cases we observed the same phenomenon: the last day's sodium and chlorine output was many times greater than on the previous days. With 2 per cent urea solution (10 ml. twice daily subcutaneously) we obtained the same result.

In some cases we succeeded in keeping the animals alive after the end of the experiment by giving them adequate food, so this phenomenon cannot be regarded as absolutely fatal. The fasting animals lived from four to nine days, and there seems to be no correlation between the duration of the experiment and the appearance of the increased sodium chloride output. No correlation was found between increased output of sodium chloride on one hand and sex, weight, room temperature, water intake, quantity of urine and nitrogen output of the rats on the other.

We have insufficient evidence to suggest the mechanism and cause of this increased output. We calculated according to Gamble, Ross and Tisdall's procedure[2] the loss of weight from the excreted amount of potassium, sodium and nitrogen and compared the data with the measured loss of weight. In most cases showing the increased sodium chloride output, the calculated loss exceeded the measured loss and this, with high serum sodium values, would suggest that the organism had some reserves of sodium chloride in 'dry' form, which are excreted after prolonged fasting. In other cases we found normal and even subnormal serum sodium levels as well. We realize that the existence or non-existence of 'dry' sodium chloride reserves is still a much discussed problem of physiology, and the evidence given above is not enough to give a positive answer. Further work on the mechanism involved is in progress.

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Thyroid Gland as a Special Factor Influencing the Conditional Disposition to Infectious Diseases

There are several old and recent observations concerning the important effect of the ductless glands on disposition against infections and infectious diseases. From this point of view the role of the thyroid should especially be emphasized.

Some authors[1] suggest a specific role (specific bacteriostatic action) of thyroxine; but the majority[2–7] assume a non-specific effect on the defence mechanism of the organism.

It is more correct to speak of conditions rather than causes producing diseases, regarding the constitution not as a static but as a dynamic unity. I reported in 1938[8] on my investigations with Reid-Hunt's method. They favour the assumption that alterations in the quantity of thyroxine in the blood are closely related to infections. My further investigations with Euler's method[9] pointed to the marked increase of the thyroxine-level in serum during infectious diseases (smallpox vaccination). It seemed that this increase was parallel with the reaction of the body to the infection and closely related to the development and stimulation of the defence mechanism. I found at the same time[10] that the patients displaying a higher thyroxine-level were rather free from the complications due to secondary infection in scarlet fever.

Starting from these observations, I administered daily to 283 patients[11] suffering from scarlet fever 50 cgm. of fresh thyroid powder from their entry to hospital until their discharge. The experiments were carried out during one season, the patients came

from the same district, and nursing, treatments, diet, etc., were identical too. The administration of thyroxine was carried out regardless of sex and age of the patients. The object of this experiment was to diminish the number and mitigate the severity of the complications both in first and in secondary infection. Thyreotoxicosis ensued only in one case. The number of control cases was 1,112.

The data are summarized below :

	Controls (per cent)	Treated cases (per cent)
Cervical lymphadenitis	25·6	6
Fever without any symptoms, total	13	3
recurrence—once	73	95
recurrence—twice	24	5
recurrence—three times	3	0
Nephritis, total	6	6
mild cases	44	70
severe cases	38	30
fatal cases	18	0

The table shows that the administration of the powdered thyroid gland did, as compared with the course of the controls, achieve an increased resistance to the complications, so much so that their absolute and relative number has diminished.

According to my observations (and in accordance with the findings of other authors[12]) the thyroid (system diencephalon – adenohypophysis-thyroid) seems to be an important point of the conditioned predisposition. Besides this, there seems to exist a close connexion between the allergic phenomena and the infectious diseases. Among other things a typical blood cholesterin-level[13], extremely mild allergic symptoms[14] in hyperthyroidism[15] and the well-known dehydrative effect of the thyroxine were observed.

It has been shown by the experiments cited that besides the antibacterial substances, conditioned disposition plays a prominent part in combating infections.

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Effect of Feeding 2-Acetaminofluorene on the Adreno-Gonadal System of Rats

The administration of *p*-dimethylaminoazobenzene to female albino rats on a rice diet has been shown to cause a cessation of the œstral cycles two weeks after administration of the carcinogenic compound[1]. The weight of the ovaries was reduced, and histologically there was an almost complete absence of corpora lutea. In male rats the weight of the testes was reduced and spermatogenesis inhibited. The weight of the adrenal was found to be smaller in the male than that in the female rats, in conformity with the findings of Hatai[2]. There was no effect on the weight or histological appearance of the adrenal gland in either sex ; 78 per cent of these rats developed hepatic tumours[3].

In a new series of experiments 2-acetaminofluorene was fed to albino and piebald rats each weighing about 100 gm. They received a semi-synthetic diet in which 17 per cent of the caloric value was derived from protein and 30 per cent from fat ; the diet was supplemented with cod liver oil and greens. The 2-acetaminofluorene was added in a final concentration of 0·07 per cent. The weight of the rats was recorded weekly. All rats grew slowly (about 0·5 gm. daily), and no difference could be found between them and rats on the control diet. Vaginal smears were taken daily from ten females in each group. Rats were killed at intervals up to the 300th day, and those developing any disease other than tumour growth were discarded. The organs were removed immediately, weighed and fixed for histological examination.

The œstral cycle both of albino and piebald rats receiving 2-acetaminofluorene ceased in seventeen out of twenty rats ; that of the control rats remained normal. Histologically the ovaries showed a considerable number of very large corpora lutea (8–12), luteinization of the stroma and an almost complete disappearance of the follicles. The weight of the ovaries in the treated and control rats was much the same. Mean weights (mgm. per 100 gm. body-weight) : controls (16), 59 ; treated albinos (13), 51·7 ; piebalds (16), 59·0. (The figures in brackets give the number of rats in the group.)

The weight of the adrenals (mgm. per 100 gm. body-weight) of the *female* rats receiving 2-acetaminofluorene remained unchanged. The mean weights were : controls (10), $42·7 \pm 2·9$; treated females : albinos (13), $42·8 \pm 4·0$; and piebald (10), $42·7 \pm 3·4$. Histologically the adrenals were in most cases normal ; some showed degenerative changes in the zona glomerulosa and there was some cyst formation in the cortex. In male rats the weight of the testes was slightly but not significantly lower than that in the controls ; spermatogenesis was found in all instances. The mean weights (gm. per 100 gm. body-weight) were : controls (17), 1·43 ; treated albinos (12), 1·25 ; and piebald (11), 1·03.

The weight of the adrenals of *male* rats was increased, approximating that of the adrenals of female rats and being about 40–50 per cent greater than those of the male control rats. The mean weights were : controls (17), $27·4 \pm 2·0$; treated male albinos (11), $38·19 \pm 3·4$; piebalds (8), $39·7 \pm 2·2$, $P < 0·01$. Histologically, the male adrenals showed hypertrophy of the cortex, most pronounced in the layers near the medulla. In some cases a well-developed juxta medullary eosinophilic zone was present.

No change in the weights of the other endocrine organs was noted. The weight of the liver increased in both sexes shortly after feeding 2-acetaminofluorene and before histological signs of tumour formation appeared. This increase seemed due to the deposition of fat in the liver cells. There were no changes in the weight of the other organs.

Eighty-five per cent of the sixty-three rats, whether albino or piebald of either sex, receiving 2-acetaminofluorene for more than twenty-one weeks developed tumours. These were located principally in the liver ; but tumours of other organs were also frequent, as found by others[4].

As in the case of *p*-dimethylaminobenzene, the administration of 2-acetaminofluorene caused cessation of the œstral cycle in female rats, but the histological picture of the ovaries was completely different; it seems that when 2-acetaminofluorene was fed, the organs which produce steroid hormones are affected. It produces in female rats a heavy luteinization of the ovaries and a consequent disturbance in the œstral cycle. The changes in the male adrenal make the gland similar to that of the females. How far these changes are related to the carcinogenic action of 2-acetaminofluorine is being investigated. In this connexion it is possibly of significance that two rats with normal œstral cycles had not developed tumours by the 300th day of the experiment.

I wish to express my indebtedness to Dr. G. Bourne for his help with the histological investigations and advice in the preparation of this paper. My thanks are also due to the British Empire Campaign for a grant supporting this work.

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Electrophoretic Patterns of Lamb Serum Before and After Transfer of Colostrum

THE increase in the serum globulins resulting from the ingestion of colostrum has been demonstrated by Howe[1] and also by Schneider and Szathmary[2], who employed precipitation methods. Jameson and Alvarez-Tostado[3] also employed electrophoretic analysis to demonstrate such an effect when cow's colostrum is fed to man or the adult rat; and, with Sortor[4], they have studied calf serum at birth and at subsequent times. Similar investigations have now been made in connexion with the passive immunity produced in lambs by the transfer of colostrum.

After dialysis the lamb sera were diluted with buffer to 1·1 per cent protein concentration, and runs were carried out in the Tiselius[5] electrophoresis apparatus in phosphate buffer (*p*H 8·0, ionic strength 0·2) to obtain the patterns shown. Figs. 1 and 2 show, respectively, sera obtained from a lamb at birth and 24 hours later, after transfer of colostrum by suckling. Fig. 3 shows, for comparison, a sample of pooled sera from a dozen normal sheep. The following points emerge. First, the lamb sera contain a rather higher percentage of α-globulin than does the sheep serum. Secondly, the β-globulin peak is scarcely distinguishable at all in the lamb sera, whereas in the sheep serum it can be distinguished as a small peak incompletely separated from the γ-globulin, and containing a sharp spike, the β-anomaly (see Longsworth, Shedlovsky and MacInnes[6]). Finally, the γ-globulin, almost entirely absent from the serum of the lamb at birth, shows a very large increase after twenty-four hours. In the normal sheep the γ-globulin content is much lower.

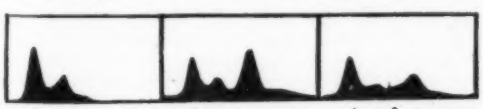

Fig. 1 LAMB, BEFORE COLOSTRUM — Fig. 2 LAMB, AFTER COLOSTRUM — Fig. 3 NORMAL SHEEP

Cathode-limb exposures are shown. Migration is from right to left. Peaks are designated thus: A = albumin; α, β, γ = α-, β-, γ-albumin; ε = ε salt boundary

Sera from eight lambs have been examined in the manner indicated, and the results shown are typical. Thus it appears that, as with other animals, so in the case of lambs the transfer of colostrum causes a remarkable increase in the γ-globulin. Advantage is taken of this transfer of γ-globulin from mother to offspring in the prophylaxis of certain diseases of the very young. In certain areas of Great Britain serious mortality occurs annually in young lambs from a disease which results from the ingestion of *Cl. welchii* type *B* organisms. The resulting enterotoxæmia is fatal to more than 90 per cent of affected lambs. The injection of in-lamb ewes in the autumn and spring with a vaccine prepared from suitable strains of the organism concerned produces antitoxin in the ewes' blood. At the time of lambing this antitoxin is present in the colostrum and the passive immunity acquired by the new-born lamb is sufficient to protect it during the first two critical weeks of life[7].

We are indebted to Mr. A. T. Glenny for advice on this work.

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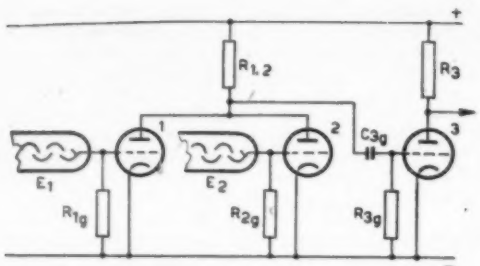
Coincidence Device of 10^{-8}–10^{-9} Sec. Resolving Power

COINCIDENCE circuits with resolving times much exceeding 10^{-8} sec. can be obtained by making use of electron multiplier tubes[1]. The device shown in the diagram is a modification of the Rossi type circuit. Ultra high-frequency electron tubes, lately developed, have been used. Because of the small inter-electrode capacities, the high mutual conductance and the small coupling resistances (50–100 ohms), the input pulses are not noticeably distorted by valves 1 and 2 and thus reach the grid of valve 3 without appreciable delay.

A difficulty which does not arise when coincidences between Geiger-Müller counters are recorded is as follows. While the input pulses obtained from Geiger–Müller counters are fairly uniform in size, the pulses arising from electron multipliers are subject to unavoidable random fluctuations. Thus the tubes 1 and 2 have to be operated at such a point of their characteristics that they tend to give equal output pulses independently of the size of the input pulse. This is achieved if tubes 1 and 2 have very sharp cut-off characteristics. In the tubes actually used the anode current was completely cut off by pulses exceeding 1 V. fed to the grid. The actual pulses received from the electron multipliers ranged between 1 and 10 V.

The tube 3 was biased so as not to respond to a pulse of 1 V. across $R_{1,2}$, corresponding to a single



input pulse, but so as to respond to pulses of at least 2 V., corresponding to coincidences.

The resolving power so far achieved is about 5×10^{-9} sec. The determination of this quantity was carried out—besides the usual method of counting random coincidences—by a direct method. The impulses of the same multiplier tube are fed into both inputs of the device, so that each impulse is recorded as an 'artificial coincidence'. If one of the leads connecting the multiplier to one of the inputs is increased by a length Δl, this side of the device will be reached by the impulse $\Delta t = \Delta l/c$ sec. later; for example, 1·5 m. corresponding to the resolving power given above. Because of this time-lag the amplitudes of the artificial coincidences decrease, and at a critical length they fade out entirely. By increasing the length of the other lead, too, the coincidences are made to reappear, demonstrating that fading-out of the coincidences was a consequence of the time-lag caused by the difference in length of the leads.

The device was designed chiefly for investigations in nuclear physics. At present, experiments similar to those of Bothe[2] are going on in the ultra-violet and visible region.

Further details will be published elsewhere.

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Electron Multiplier Tube of Large Effective Cathode Surface Area

Electron multiplier tubes have been used in this Laboratory for particle counting[1], and their extremely short impulse period[2] makes them very useful research tools[1]. One disadvantage of these tubes is their small effective surface.

One method for increasing the effective surface is the following: the electron-optical image of a cathode of large surface area is projected on the first multiplying electrode of a multiplier tube of the usual dimensions. In this way a tube similar to the partition-type multiplier of Zworykin and Rajchman[4] was designed. The electrons are emitted by a cathode of spherical shell shape, and the image of the surface of the cathode is projected on the first multiplying electrode by an electrostatic lens. The cathode subtends an angle of 90° viewed from the focus of the lens. In this manner an effective surface of 100 cm.[2] can easily be obtained.

The centre of curvature of the cathode is at the focus of the lens; the transit time of the electrons between cathode and first multiplying surface is very nearly the same for all electrons, regardless of their point of origin. This is most important in coincidence devices of very high resolving power.

The dimensions of the lens electrodes and the potentials thereon can be adjusted so that practically every electron emitted by any point of the cathode can reach the first multiplying stage without being captured by intermediate electrodes or repelled to the cathode.

In the experimental tubes, electrons could be released from well-defined points of the cathode. Some of the tubes were enlarged models, having electrodes covered by a thin layer of fluorescent material. In this way the incidence of the electrons was made visible. This method was very useful, especially in the first qualitative experiments.

Another possible way of increasing the effective surface of the electron multiplier tubes is to increase the length of all electrodes of a circular-type multiplier[4]. This method, however, would increase the capacity of the collector plate, which is a disadvantage from the point of view of the impulse period of the tube and the amplitude of the voltage pulse.

The development of the tube for the purpose of researches on cosmic radiation is going on.

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Variation of Thermo-electric Power of Electronic Semi-Conductors with Vapour Pressure

When the principle of mass action is applied to explain the effect on the electrical properties of certain electronic semi-conductors having an ionic lattice structure, produced by varying the vapour pressure of one of the constituents of the substance, it is found that

$$n_f = KP_e^{1/n}, \qquad (1)$$

where n_f is the concentration of charge carriers, which may be free electrons or holes, K is an equilibrium constant, P_e is the vapour pressure, and n is a constant, negative for excess and positive for defect semi-conductors, which can be deduced from the chemical structure of the substance.

If this equation is introduced into the usual free-electron theory for the thermo-electric power of semi-conductors[1], equations may be obtained corresponding to the empirical equation

$$\frac{dE}{dT} \pm \text{const.} = -C_T \log P_v, \qquad (2)$$

where dE/dT is the thermo-electric power, taken as negative for excess and positive for defect semi-conductors, and C_T is a constant for a particular substance. From the theory C_T may be identified with $k/|e|n$, where k is Boltzmann's constant and e is the electronic charge, and a value for n thereby obtained.

Little experimental work has been performed on this subject except for a few spot readings by Wagner and his co-workers[2,3,4], made to test Wagner's

differential thermo-electric circuit concept[5]. Assuming that these results obey equation (2) above, values of n have been calculated and are shown below for comparison with the values obtained from conductivity measurements:

Substance	Theoretical value of n	Value from conductivity measurements	Value deduced from thermo-electric measurements
Cu_2O	8	7	8·8
NiO	6	4	4·8
ZnO	6	4·3	6·1

The experimental results are not sufficiently comprehensive or accurate for a critical comparison, but it is noteworthy that in each case the value of n calculated from thermo-electric properties is in closer agreement with the theoretical than that obtained from conductivity measurements.

Experiments on various semi-conductors are being carried out and the results to date show that equation (2) is, in fact, obeyed. Details of the equations and their derivation will be published later.

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Nuciear Models

In a recent paper[1], H. A. Wilson has proposed a spherical shell model for the nucleus of a heavy atom. In this model the nucleus is represented as an inextensible, flexible, spherical shell the shape of which is maintained by the Coulomb forces between the protons. The radius of the shell is assumed to be $R = R_0 A^{1/3}$, where R_0 is a constant and A is the total number of nucleons in the nucleus, and, according to Wilson, the frequency of the nth mode of vibration of the shell is given by

$$\omega = \omega_0 (n^2 + n - 2)^{1/2},$$

where $\omega_0 = eZ/2R_0^{3/2}M^{1/2}A$.

A model of this type has much to recommend it, but calculations we have recently performed indicate that it is not entirely satisfactory. To give a density of energy-levels of 0·1 V. for an excitation energy of 6,800 kV. in uranium requires that $\hbar\omega_0$ should be about 378 kV. for this nucleus. The value of $\hbar\omega_0$ for any other nucleus can then be obtained by multiplying by the appropriate factor Z/A as indicated by the definition of $\hbar\omega_0$. When the value for $A = 120$ is used to calculate the spacing of the energy levels for an excitation energy of 8,000 kV., we obtain a spacing of 0·05 V., which is definitely not in agreement with the experimental evidence. This fact cannot, however, be used as an argument against Wilson's *general* idea, but only against the extrapolation of the condition of the inextensibility of the shell to high-excitation energies.

On the other hand, calculations based on the assumption that the neutrons and protons forming a heavy nucleus are contained in the same spherical volume leads to a value $R_0 = 1{\cdot}56 \times 10^{-13}$ cm. from which the nuclear radius can be evaluated. This value is not in agreement with the value R_a of the same constant derived from the many-body theory of α-decay[2]. The discrepancy between the two values can, however, be removed by modifying the type of nuclear model used. The use of the statistical method of calculation imposes no restriction on the location of the volumes occupied by the protons and neutrons forming the nucleus, so we are at liberty to construct a model in which they occupy different regions of space.

If for simplicity we assume that the number of neutrons is equal to the number of protons, then they will occupy equal volumes. The simplest way of constructing a nuclear model in which the protons occupy a volume equal to that occupied by the neutrons but not identically situated is to consider that the neutrons form a central spherical core which is surrounded by a spherical shell of protons. The radius of the inner core is then $R_0 A^{1/3}$, while the external radius of the whole nucleus corresponds to $R_a A^{1/3}$ of the α-decay theory. Since the volumes of core and shell are equal, we have immediately that $R_a^3 - R_0^3 = R_0^3$, so that $R_a = \sqrt[3]{2}\, R_0$.

Taking the value $R = 1{\cdot}56 \times 10^{-13}$ cm. given by our calculations, we then obtain $R_a = 1{\cdot}97 \times 10^{-13}$ cm., which is in good agreement with the value 2·0(5) cited by Bethe. Furthermore, taking R_0 to be the radius of a proton, we find by a simple calculation that the proton shell in heavy nuclei is only one proton thick, so that each proton in the shell is in contact with at least one neutron of the inner core.

The details of the calculations will be published shortly in the *Proceedings of the Cambridge Philosophical Society*.

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Sept. 17.

[1] *Phys. Rev.*, **69**, 538 (1946).
[2] Bethe, H. A., *Rev. Mod. Phys.*, **9**, 85 (1937).

Spectrum of White Dwarfs

In a recent paper, Blackett[1] has suggested that the Zeeman effect might be responsible for the peculiar absence of metallic lines in the spectra of most of the white dwarfs and may also contribute to the broadening of the hydrogen lines. Since this suggestion was given as one more argument in support of the theory of the magnetic field of massive rotating bodies, it may be of interest to point out that considerations of the internal constitution of white dwarfs seem to offer an alternative explanation of the peculiar features of their spectra.

Such an explanation may, in fact, be afforded by the strong separation of elements in the high gravitational field of white dwarfs. It has been shown[2] that we must expect in a white dwarf a core of heavy elements surrounded by a hydrogen layer. (In this paper I had assumed the inherent β-radiation to be forbidden (in the sense of the selection rules); but, as shown below, the results of Bethe and Critchfield are not in contradiction with astrophysical evidence.) Between them exists a mixed layer the thickness of which, proportional to the temperature and inversely proportional to the acceleration of gravity, is very small compared with the radius of the star. The decrease in concentration of heavy elements in the hydrogen layer is extremely rapid, so that the con-

centration of metals in the atmosphere could be many hundred negative powers of ten.

If the hydrogen layer is sufficiently thick, the metal lines should, thus, not appear in the spectrum. This case should correspond to stars like Wolf 489 or o_2 Eridani *B*. The presence of metal lines in the spectra of some white dwarfs is only possible when the hydrogen layer is very thin and the concentration of heavy elements great at the bottom of the non-degenerate outer part of the star. Convection currents, such as those found by Unsöld, could then carry heavy elements to the very surface of the star. This seems to be the model for a star like Van Maanen 2. The great proximity of heavy elements to the surface, and such convection currents, would be sufficient to explain the presence in the spectrum of lines of light metallic elements like calcium. The theory of production of lines in stellar atmospheres also indicates that collision damping should be sufficient to explain the broadening of lines of Ca^+. The weakness of lines of heavy elements such as iron should be connected with the strong separation of heavier elements in the gravitational field.

Such considerations about the thickness of the hydrogen layer agree also with calculations on the energy production. The main source of energy is represented by the reaction

$$^1H + {}^1H \rightarrow {}^2D + e^+,$$

and the ensuing reactions, the rate of which has been calculated by Bethe and Critchfield[3]. This rate is much greater than that corresponding to processes in the mixed layer, analogous to those occurring in the sun. For the white dwarfs we get in this manner an estimate for the thickness of the hydrogen layer which should be responsible for the actual energy production and which, in fact, turns out to be large compared with the thickness of the mixed layer for o_2 Eridani *B* and Wolf 489, and extremely small in the case of Van Maanen 2.

A more detailed account of the above ideas based on a fuller discussion of the constitution of the various types of white dwarfs will be given in a later paper. I wish to thank Prof. B. Strömgren for stimulating discussions on the subject.

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Nov. 12.

[1] Blackett, *Nature*, **159**, 658 (1947).
[2] Schatzman, *Ann. Astrophys.*, **8**, 143 (1945).
[3] Bethe and Critchfield, *Phys. Rev.*, **54**, 248 (1938).

The Bikini Bomb and the Seismology of the Pacific Region

Gutenberg and Richter[1] have recently published numerical travel-time data for *P* waves recorded at eight stations in the United States, following the detonation of an atomic bomb over Bikini on 1946 July 24. They point out the importance of these waves in checking travel-time tables, and use the results to test the accuracy of earlier tables[2] published by them. The purpose of this note is to compare the data with the Jeffreys – Bullen tables[3]; and, further, to investigate the precision with which the result of this comparison may be applied to the question of the structure of the Pacific region.

The eight recording stations were at distances from the place of detonation ranging from 69° to 78·6°. The excesses of the 'observed' travel-times of the first *P* waves over the times given in the Jeffreys–Bullen tables are found to be −1·6, −0·4, −1·6, −2·1, −1·1, −2·7, −1·1, −0·4 sec., yielding a mean of −1·4 ± 0·7 sec. (the time of detonation of the bomb is known within 0·1 sec.). To this must be added the mean ellipticity correction[4] of −0·4 sec., giving a mean residual of −1·8 ± 0·7 sec. (It is assumed that the data of Gutenberg and Richter relate to geocentric and not geographic latitudes.)

A residual of this order is to be expected, because of special features of the Pacific structure[5]. Thus the results of the Bikini bomb suggest no improvements to the Jeffreys – Bullen tables for epicentral distances of the order of 70°–80°.

On independent grounds, the Jeffreys – Bullen *P* tables are indicated as being accurate within standard errors of the order of 0·4 sec. for 'average' earthquakes. This would indicate that the travel time of *P* waves between Bikini and stations in the United States is less by 1·8 ± 0·8 sec. than the corresponding travel-times for average earthquakes (in which epicentres are in continental regions). This figure may be of some importance in subsequent investigations of the Pacific region.

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Sept. 12.

[1] *Trans. Amer. Geophys. Union*, **27**, 776 (1946).
[2] *Gerlands Beitr. z. Geophys.*, **54**, 94 (1939).
[3] British Association, Gray Milne Trust (1940).
[4] Bullen, *Mon. Not. Roy. Ast. Soc., Geophys. Suppl.*, **4**, 143 (1937).
[5] See, for example, Bullen, "Theory of Seismology", 203, 209 (Camb. Univ. Press, 1947).

Sublimation in the Atmosphere over the Oceans

THE results of measurements of the highest temperature at which ice particles were formed in a Wilson expansion chamber, using air at the Jungfraujoch Observatory, were recently reported[1]. Similar measurements have now been made in the air over the Atlantic and Pacific Oceans on board R.M.S. *Rangitiki*, N.Z.S. Co., during a voyage from London to Wellington (May 10–June 16, 1947).

Measurements made in the 'Horse Latitudes', in the North-East Trade Wind in the Atlantic and in the South-East Trade Wind in the Pacific, gave invariably − 41·2° C. as the ice threshold.

From these measurements and the previous ones at the Jungfraujoch it may be concluded with all probability that there are no nuclei of sublimation in the atmosphere at the cirrus-level, except possibly in cases where they are carried up from ground-level by the violent convection in thunderstorms originating over areas where the air contains industrial pollutions. In such cases the ice threshold will be some 9° C. higher, and owing to this, such areas might be more subject to showers.

My thanks are due to Sir Nelson Johnson for the loan of the necessary apparatus and to Capt. E. Holland of R.M.S. *Rangitiki* for providing facilities for the measurements.

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June 23.

[1] [*Nature*, **160**, 198 (1947)].

Occurrence of Troilite

MINERALS described from the Daggafontein Gold Mine[1] were later made available to me for detailed study. They included typical brownish magnetic tabular pyrrhotite crystals 2·5 mm. wide by 0·5 mm. thick, showing steep pyramid, prism and basal pinacoid faces. Some were intimately associated with black nodules of a hydrocarbon and grey chlorite streaks in lumps of white, finely crystalline dickite.

One large and one smaller crystal identical in form with the majority were more like pyrite in colour and practically non-magnetic. These crystals contained 62·4 per cent of iron, which is nearer to troilite (usually only found in meteorites) than to pyrrhotite in the series pyrrhotite-troilite. The typical pyrrhotite contained 60·31 per cent of iron (kindly analysed by Mr. T. Steele). Troilite has been recorded from the Witwatersrand previously[2], but the occurrence of two distinct varieties of the pyrrhotite series together is worthy of note.

It is suggested that detailed studies of pyrrhotite crystals will prove terrestrial troilite to be more abundant than was hitherto considered.

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Natal University College,
Durban. Sept. 18.

[1] Whiteside, H. C. M., *Trans. Geol. Soc. S. Africa*, **44**, 121 (1940).
[2] Mendelssohn, E., *J. Chem. Soc. S. Africa*, **45**, Nos. 4 and 5 (Nov.–Dec., 1944).

Minimum Perceptible Energy and Brownian Motion in Sensory Processes

FOR all sense-organs there exists a lower limit to the minimum perceptible energy, which is set by the Brownian motion in the sensory equipment itself. The minimum perceptible energy must be so large that only a small number of spontaneous excitations by thermal motion can occur. The quantitative relations have been estimated tentatively in certain cases.

(*a*) *Vision.* The minimum perceptible stimulus corresponds to the absorption of a single light quantum by a molecule of visual purple in each of several retinal rods—about five according to Hecht *et al.*[1], or two according to Van der Velden[2]. Suppose that one rod only need be excited. The number of spontaneous thermal excitations will be approximately $q = \frac{N}{\tau} \exp - E/kT$ per sec., where E is the energy necessary to decompose a visual purple molecule and $\exp - E/kT$ is the probability that the essential part of the molecule has a thermal energy greater than E at a given moment (at 37° C., $kT = 4{\cdot}3 \times 10^{-14}$ erg). τ is the mean life-time of a given distribution of the energy; every second we have $1/\tau$ 'new' distributions. For molecules, τ is about 10^{-13} or 10^{-14} sec. (see ref. 3). Finally, N is the number of sensitive molecules in one cell. For applications, we can write our formula as

$$E = 2{\cdot}30\ kT \log\left(\frac{1}{q}\frac{N}{\tau}\right), \quad . \quad . \quad . \quad (1)$$

For the visual process, the calculation of the energy E which is necessary in order to avoid spontaneous excitations can be made with fair accuracy. For the rods, N is 4×10^9 (ref. 4). Strictly speaking, this was found for a frog retina, but it follows from the high sensitivity of the human retina (see ref. 2) that the concentration in dark-adapted human rods cannot be lower than about 10^9 molecules per rod. Hence $N/\tau = 10^{23}$, and according to formula (1) E must be $2{\cdot}3 \times 23\ kT = 53\ kT$ in order to experience less than one excitation per second. Even if ten excitations a second are admitted, E is not materially changed.

For the visual process the limit of 53 kT does not offer anything new, because from the sensitivity curves of the receptors it follows already that E is probably more than 70 kT. Moreover, the spontaneous excitation of only one rod does not give rise to the *sensation* of light; two excitations which occur within about 0·05 sec. are necessary[1,2]. (S. Hecht *et al.* arrive at about five quanta, but since the two-quantum hypothesis, developed by v. d. Velden, was checked by other observations, this number seems more probable.) Of course, the number of spontaneous *sensations* will be greatly reduced in this way, and this may be a reason why this remarkable 'coincidence mechanism' is present in the eye.

(*b*) *Excitation of a Nerve.* For other processes a similar reasoning can be applied, if the various constants are properly defined. As an example, we will consider the excitation of a nerve. For E should be inserted the energy which is necessary for the depolarization of an area O which is large enough to propagate itself. A reasonable value[5] for the energy per cm.² is about 4×10^{-4} erg, so that O should be 3×10^{-9} cm.² to have $E = 30\ kT$. (This value for O will only be assumed as a first approximation.) τ is unknown; it cannot be more than 0·001 sec., since after excitation the normal configuration is restored in about 0·001 sec.[6]. For N, the number of units any of which can start an excitation we should insert the total area of a nerve, divided by O. For a nerve, length 30 cm., diameter 5μ, $N = 3 \times 10^7$. From these data we find that in this case E should be about 25 kT (or more). Then $O = 3 \times 10^{-9}$ cm.² or 2×10^{-4} cm. of the nerve. It is not probable that an area of depolarization of 2×10^{-4} cm. of a nerve is sufficient to propagate itself. This means that the nerve considered is very stable with regard to thermal excitation, and that 25 kT does not suffice for excitation. If the mechanism of the nerve were known in more detail, it would be possible to give a more elaborate discussion. Since the probability of excitation depends largely on the energy E, however, we can safely assume that for the nerve and probably *several other processes* the limit will be at least 25 kT.

(*c*) *Hearing.* For the process of hearing the limit of about 25 kT for one sense cell, mentioned above, is more important. This will be discussed in more detail in the near future in a paper to be published in *Physica*, but the leading idea is as follows. The minimum audible energy is some 300 kT. According to the present views, this energy is distributed over at least 1·5 mm. of the basilar membrane, or 1,000 sense-cells, so that one cell only receives 0·3 kT. This is obviously much too far below the limit mentioned in this letter to be accounted for by experimental errors. From this discrepancy interesting conclusions about the mechanism of hearing can be drawn.

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[1] Hecht, S., *et al.*, *J. Gen. Physiol.*, **25**, 819 (1942).
[2] Velden, H. A. v. d., *Physica*, **11**, 179 (1944).
[3] Fowler, R. H., and Guggenheim, S. A., "Statistical Thermodynamics", 529 (Cambridge Univ. Press, 1939).
[4] Broda, H., Goodeve, C. F., and Lythgoe, R. J., *J. Physiol.*, **98**, 397 (1940).
[5] Hill, A. V., "Chemical Wave Transmission", 26 (Cambridge Univ. Press, 1932).

Occurrence of *Elminius modestus* (Darwin) in British Waters

THE observations on the existence of *Elminius modestus* on British coasts reported by Mr. M. W. H. Bishop[1] have been confirmed and extended in an independent investigation commenced by Messrs. Blacklock and Macarthur, Glasgow, in January 1946.

Some of our records of this species are listed below. It will be noted that *Elminius* is widely distributed and abundant on the south-east coasts of England, and, in fact, has been taken at every point which we have visited, where a suitable substratum exists, between Norfolk and Dorset. It occurs intertidally, mainly below mean tide-level, but may be found higher in protected situations, and also at depths down to at least 5 fathoms. Its habitat and settling behaviour appears to be in British waters exactly as described by Moore in New Zealand[2]. In the Essex Rivers it competes very successfully with *Balanus balanoides* in the lower part of the intertidal zone, but the latter has the advantage of slightly earlier settlement and longer life; under water it is rather less successful in competition with *B. crenatus* and *B. improvisus*. *Elminius* ranks as a dominant littoral organism in the estuaries of the Colne, Blackwater, Crouch and Thames.

We have also considerable data suggesting that this species is mainly confined to the south and east coasts of Britain. Careful examination of the foreshore and of underwater exposure panels has failed to reveal any *Elminius* at our stations at Brixham, Devon; Pierhead, Liverpool; Port Erin, Isle of Man; and Portree, Isle of Skye, though a sparse settlement has recently been recorded at Neyland, Pembrokeshire. Less rigorous examinations of material from Conway and the Blyth Estuary, Northumberland, suggest its absence from these localities.

Elminius modestus has a longer season of reproduction and continuous settlement than any other barnacle in British waters, and the spat has been observed to reproduce early in the same season in the Rivers Blackwater and Crouch[3]. Indeed, it seems theoretically possible from our growth-rate and breeding data that two complete generations may occur during summer in such an estuary, since fully developed nauplii may be found at any stage after a rostro-carinal diameter of 4–5 mm. has been attained.

RECORDS OF *Elminius modestus* (Darwin)

Locality	Occurrence	Earliest record
Brancaster, Norfolk	Not common	August 1947
Lowestoft, Suffolk	Frequent	August 1947
Rivers Colne and Blackwater, Essex	Abundant	Settlement April 1 to October 10, 1946
N. Fambridge, Essex	Abundant	October 1945
Thames, below Gravesend	Common	September 1947
Thanet coast, Kent	Occasional	September 1947
Brighton, Sussex	Frequent	October 1947
Poole, Dorset	Common	June 1947
Neyland, Pembrokeshire	Occasional	August 1947

The present distribution of *Elminius* does not indicate clearly a recent dissemination from one or more centres, but appears to be that of a well-established species. It is abundant in the favourable environment of shallow muddy estuaries with high summer temperatures, rarer on sandy and chalky foreshores and absent where there is considerable wave exposure[3]. The very intense barnacle-fouling experienced in these muddy estuaries is due in considerable degree to *Elminius*, and in view of the frequent coastal traffic, its absence from ports in the south-west and west of England may indicate an unfavourable environment. It is possible that lack of interest in the Cirripedia until recently, the paucity of faunistic work on the south-eastern shores of Britain, and a superficial resemblance to *B. balanoides* have combined to give the immigrant freedom from recognition for some years. The simultaneous records of Bishop and ourselves in 1945 at places as distant as Fambridge and Chichester support this view.

A comparison between the introduction of *Elminius modestus* and the invasion of the Essex rivers by *Crepidula fornicata* in about 1880 may be of interest. The latter species has succeeded in colonizing submarine levels of the same environment and, spreading rapidly at first, achieved a similar distribution over a period of forty–fifty years[4].

A fuller account of its distribution, development and breeding habits in British waters will be published at a later date.

D. J. CRISP
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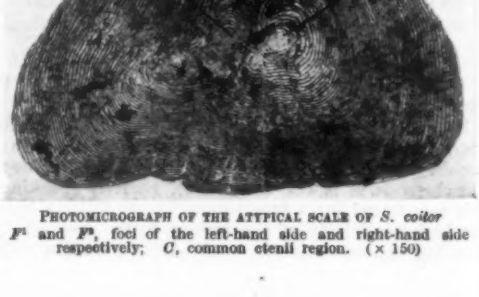
Oct. 17.

[1] *Nature*, **159**, 501 (1947).
[2] Moore, L. B., *Trans. and Proc. Roy. Soc. New Zealand*, **73**, 315 (1944).
[3] Cf. Darwin, "Monograph on Cirripedia", 348 (1854).
[4] Orton, J. H., *Proc. Roy. Soc.*, B, **81**, 468 (1909).

An Atypical Scale of *Sciæna coitor* (Hamilton)

IN the course of studies of the structure and development of ctenoid scales of certain Indian fishes[1], an atypical scale was encountered (see accompanying illustration). It was found on the body of a male *Sciæna coitor* which measured 20 mm. in length. It was located in the middle region of the trunk on the fifth row below the lateral line. The neighbouring scales and those belonging to the other regions of the body were also critically observed but none of them showed any breach of normal development.

The most interesting feature of this scale was the presence of two foci instead of one. Both the foci lie in the middle zone of the scale. The focus of the right-hand side (F^2) was circular, while that of the left-hand side (F^1) was more elongated. The focus of the right was bounded by more or less complete concentric circuli. The circuli encircling the corresponding focus were extremely longitudinal and consequently incomplete. Of the two foci, the right



PHOTOMICROGRAPH OF THE ATYPICAL SCALE OF *S. coitor*
F^1 and F^2, foci of the left-hand side and right-hand side respectively; *C*, common ctenii region. (× 150)

one was larger than the left. Ctenii were at the formative stage, the deposition being laid. Seven of them (*C*) were conspicuous. They were devoid of basal components. The ctenii region occurred in such a position as not to come exclusively under the ambit of any particular focus, since it lay in the middle line passing between two foci. Eight radii had appeared and they extended centripetally towards the foci.

The foci, on being surrounded by their own circuli, could be taken to represent two scales at the incipient developmental stage. It might be imagined that at an early period of development, owing to some developmental irregularity probably caused by arrested growth, the inferior layers of these two incipient scales were fused to form a common lower layer. The hyalodentine of each of them, which was already calcified, retained its identity as the focus in spite of fusion of their lower layers. Thus the presence of two foci was explained. Thereafter by this union the rest of the features which were laid down by the activity of the inferior layer were common to both foci. Thus it appeared that two incipient scales merged so as to give the appearance of a single scale. The nature of ctenii being common to both the foci could be explained, because the ctenii were formed as delayed deposition, as previously shown by Ganguly and Mookerjee.

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Oct. 7.

[1] Ganguly, D. N., and Mookerjee, S., *Proc. Nat. Inst. India* (in the press).

Resistance to Inanition in Grass Seedlings

SOME years ago I found that grass seedlings which were in competition with other grasses older than themselves were able to remain alive for several months, although growth, in the sense of increase in size, could not take place[1]. In these experiments the competition of the older plants operated by the removal of mineral nutrients from the soil surrounding the young seedlings, and by the exclusion from the latter of light. The tolerance of the latter was so pronounced that an inquiry was started into the general ability of grass seedlings to survive inimical conditions.

The results can be summarized as follows:

(i) Increase in size of seedlings may be completely inhibited by a lack of mineral nutrients, but with this factor alone operating and with full light, the seedlings survive indefinitely.

(ii) Seedlings from which light is completely excluded remain alive for periods determined by the temperature prevailing; for example, at 22° C. survival does not exceed 20 days, whereas at 9° C. survival may extend to 18 weeks. At the end of this time, on the admission of light, chlorophyll is formed and normal development can proceed. Resistance to inanition is independent of the food reserves in the caryopses, and of the presence of chlorophyll. This behaviour is shown by seedlings grown *in vitro*, and in soil; and seedlings from seeds sown in the autumn may not appear above the soil until the following spring. Low mineral nutrition, and the admission of light at low intensities, or for intermittent short periods, increases the longevity of the seedlings.

(iii) The well-known ability of seeds to develop after desiccation during the initial stages of germination is shared in a lesser degree by grass seedlings which have developed two or three leaves. After six days air-drying at 14° C., survival is approximately 20 per cent, and the survival period can be considerably extended by intermittent moistening of the seedlings.

(iv) All the commoner species of British grasses show this ability to resist inanition and desiccation; but no dicotyledonous species so far tried has possessed it.

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[1] Chippindale, *Ann. Appl. Biol.*, **19**, 221 (1932).

Mauthner's Cells in the Larvæ of Anuran Amphibia

IN a previous communication[1], confirming the occurrence of Mauthner's cells in the larvæ of *Rana*, each axon process was described as dividing into two similar fibres, each of which decussated separately. Subsequent work, while confirming the existence of this condition in other batches of material, has shown that it is not invariable. In many instances the axon process remains undivided, at least in the region of the medulla oblongata.

Although variability of this type is apparently unique, my records show clearly that it is not associated with any marked differences in the maintenance of equilibrium, the locomotory habits and general behaviour of living larvæ.

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[1] Willis, A. G., *Nature*, **159**, 410 (1947).

Demonstration of the Heart Beat

I SHOULD be interested to know whether any physiologist who is also a smoker has noticed the following phenomenon, and whether it has been previously reported. Choose a quiet room, fill the mouth with tobacco smoke, and blow gently out through a very small aperture between pursed lips. Careful observation of the fine jet of smoke will show that it has a definite periodicity—that of the heart beat. With a delicately controlled jet it is even possible to make the heart blow a smoke ring at each beat.

It is evident that the heart is giving a pulse of pressure to the air in the mouth, just as does the hand when tapping the stretched diaphragm of the well-known box used for the demonstration of smoke rings. The pressure pulse may be communicated directly through the heart wall, or perhaps it is given by way of the arteries in the throat and mouth. It is possible that observation of air-pressure in the mouth by means of a quick-acting micromanometer might give useful information about the condition of the arteries responsible.

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NATURAL AMŒBIC INFECTIONS IN LABORATORY RODENTS

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DURING the course of chemotherapeutic experiments on *E. histolytica*, the need for a suitable laboratory host for this parasite became early apparent. Jones[1] has obtained some interesting results by using young rats weighing 20–33 gm. for this purpose. We were, however, rather perturbed by the degree of infection with amœbæ which occurred in the animals of an age suitable for experimental work, when examined by us as a preliminary to their use in experiments of the above nature.

We therefore investigated the incidence and degree of natural infection in other laboratory animals which might prove suitable hosts. For this purpose we made a careful examination of the intestinal contents of white rats, cotton rats (*Sigmodon hispidus*), hamsters (*Cricetus auratus*) and Orkney voles (*Microtus orcadensis*). Wenyon[2] has indicated that amœbic infection is widespread in the animal kingdom. The amœba of the rat and mouse was first described by Grassi[3] and both were named by him *E. muris*. Numerous investigations on the rat parasite have since been recorded and in our case were made for comparative purposes. Wenyon[4] gave a clear description of *E. muris* in the mouse, pointing out that in fresh and stained preparations it had a striking resemblance to *E. coli*, as previously noted by Grassi. The character of its nucleus in stained sections agreed closely with Schaudinn's description for the latter parasite. Brug[5] described a second and rarer amœba present with *E. muris* in the rat intestine which resembled *E. histolytica*. He considered the new parasite was identical with the amœba of human dysentery and that the rat was a possible source of amœbic dysenteric infection for humans. Kessel[6] later introduced a new nomenclature for the amœbæ present in rats and mice, but his views have not yet met with general acceptance. Recently Neal[7] has described the presence of an amœba in golden hamsters differing from *E. histolytica*, with which he was able to infect young rats by feeding cystic forms of the organism. The presence of amœbæ in the cotton rat and the Orkney vole has not so far been reported. All the animals which we examined were normal specimens of known age except in the case of a few hamsters which had been used to maintain a strain of *L. donovani*.

Methods

Examination of cæcal contents was carried out after killing the animal with chloroform, and a rough count of the number of amœbæ present in fresh specimens made. Ten minutes was spent on each of six coverslip preparations before the absence of amœbæ in any animal was recorded. Examination was also made for the presence of cysts after concentration of the whole contents of the large intestine by the method of Yorke and Adams[8], and sometimes further concentrated by the zinc sulphate method. By means of a micrometer eyepiece, measurements of a hundred trophozoites and fifty cysts were made of the amœbæ of each animal species. Stained smears of both forms of the parasite were also made, as well as of sections of cæcum, especially in those cases in which infections were heavy, the latter in order to note whether any lesions of the gut wall were present. Smears fixed in Schaudinn's solution were stained by the iron-hæmatoxylin method of Heidenhain and counter-stained with aqueous eosin. Prolonged differentiation was necessary to demonstrate nuclear structure. Sections were fixed in Bouin's solution and stained by the above method.

Distribution of Amœbæ

It was found, as shown in Table 1, that approximately 50–70 per cent of each animal species was infected with trophozoites; but the percentage of animals with cystic forms was considerably less. The intensity of infection varied greatly from animal to animal in each species. In general, one to twenty trophozoites were seen per hundred microscopic fields, using 1/6 in. objective and ×6 eyepiece, but in the case of a few white rats as many as thirty parasites were present per field. In hamsters and voles the highest number recorded was five to ten per field, but in cotton rats trophozoites were nearly always scanty and the heaviest infection seen was about one per field, while cysts were even more difficult to find in that animal.

TABLE 1. INCIDENCE OF AMŒBIC INFECTION

Animal	No. examined	% with trophozoites	% with cysts
Hamster	58	53	24
Orkney vole	41	68	19
Cotton rat	72	55	5
White rat	35	65	—

Quite early it became apparent that if trophozoites were very scanty or absent, the chance of finding cysts was negligible, and our methods were modified accordingly. Age groups from two weeks up to more than a year were included in these examinations. In no case was infection detectable in the youngest group; but in the case of white rats, about half were infected at the age of three to four weeks. Cotton rats and hamsters were infected at the same age, and the heaviest infection recorded for hamsters was seen in an animal four weeks old. Voles were not found to be infected before six weeks of age and only a few younger animals were available for examination. The presence of amœbæ in any of these animals was unaccompanied by symptoms or lesions of the gut wall.

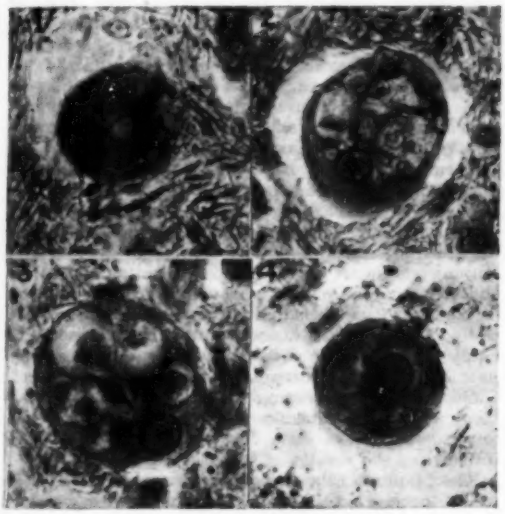
TABLE 2. DIAMETERS OF AMŒBÆ (IN MICRONS)

Host	Trophozoites Limits	Trophozoites Mean	Cysts Limits	Cysts Mean
Hamster	14·4–28·0	18·8	16·2–24·0	20·4
Cotton rat	13·8–26·4	18·6	14·4–19·2	15·6
Vole	13·2–24·0	18·3	12·5–17·6	14·6
White rat	16·3–32·5	20·1	13·7–24·0	17·3

As shown in Table 2, considerable variation in size of trophozoites was encountered, but their mean diameters for the different animals were approximately the same. The variation in size of cysts was not so great, and again there was considerable agreement among the different species of animal. The average diameter of the cysts recorded by us for the hamster is greater than that for trophozoites of the same animal, a result which may have depended partly on the number measured and partly on the considerable variation among trophozoites themselves.

Morphological Characters

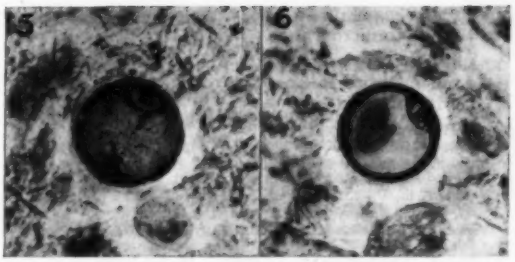
The following description of morphological characters is applicable to the parasites from the different hosts. In living preparations much of the activity of the amœbæ is confined to changes in shape without directive locomotion, pseudopodia being simply extended and withdrawn. The pseudopodia are at first composed of clear ectoplasm into which the endoplasm later streams. Several clear pseudopodia composed solely of ectoplasm may be extended in quick succession in several directions, so that at one particular moment an amœba may possess more than one pseudopodium. The ectoplasm is a narrow layer and is frequently only made visible during the formation of pseudopodia, while the endoplasm is granular and contains many vacuoles. The food matter is extremely variable and was seen to include bacteria, plant debris, and degenerated cells, probably of other Protozoa. The nucleus is also visible in the living amœba as a clear spherical inclusion surrounded by refractile granules.



Figs. 1–4. PHOTOMICROGRAPHS OF TROPHOZOITES FROM THE FOUR DIFFERENT HOSTS. × 1,000
1. HAMSTER. 2. COTTON RAT. 3, WHITE RAT. 4. VOLE

In fixed and stained preparations (Figs. 1–4), the ectoplasm is again indistinct; the endoplasm is usually vacuolated. The spherical nucleus generally has a uniform peripheral chromatin layer composed of fine granules in contact with one another and closely applied to the nuclear membrane; but in a few instances one or two condensed masses of chromatin were situated in the peripheral layer. The karyosome is compact and eccentrically placed within the nuclear membrane. The region between the karyosome and this membrane is occupied by a fine achromatic network and may contain a few chromatin granules.

In fresh cysts, chromatoid bodies were not noted with certainty. The maximum number of nuclei visible after treatment with iodine was eight; the karyosomes were usually eccentrically placed. The presence of glycogen was demonstrable in the usual way; it appeared to be more abundant in those



Figs. 5 and 6. PHOTOMICROGRAPHS OF BINUCLEATE CYSTS FROM TWO HOSTS. × 1,000
5. WHITE RAT. 6. HAMSTER (ONE NUCLEUS OUT OF FOCUS)

cysts with few nuclei. In stained preparations of cæcal contents of the hamster, vole and white rat, binucleate cysts containing a large vacuole were encountered (Figs. 5 and 6) similar to those described by Wenyon in mice. In mature cysts the nuclear characters were similar to those of the trophozoites.

Attempts at cultivation were consistently made with fæces containing trophozoite forms from all sources as well as with concentrates of cysts on the media described by Dobell and Laidlaw[9], but without success. At best, survival was obtained over a 48-hour period in some cases, but subculture never succeeded, and amœbæ loaded with starch grains so common in stock cultures of *E. histolytica* and *E. coli* were not observed. The addition of acriflavine in a final concentration of 1/10,000 to the cultures, which is sometimes successful in restraining overgrowth of accompanying bacteria as described by these authors, did not materially improve our results, nor did cultivation at temperatures of 37° and 40° C.

Summary

It is clear from these numerous examinations of fresh and stained preparations that the amœbæ present in the different animal species investigated are very similar, as judged by size, distribution and morphology. Red blood cells were never seen within these amœbæ, which was in marked contrast to the findings when experimental *E. histolytica* infections were produced in the same animals. Parasites were present in the cæcal crypts, but no damage to the gut wall was ever apparent, although this is common in infections with *E. histolytica* in these animals. The amœbæ are therefore not pathogenic in their natural habitat and in this respect as well as in morphological characters possess many of the characteristics of *E. coli*. It is quite probable that the parasites described from these different sources are strains of the same species. We have not, for obvious reasons, attempted any cross-infection experiments. It is of interest that the cotton rats showed this amœbic infection shortly after their arrival in Great Britain although segregated from all other animals here. It was of further interest that a small percentage of these same animals should be found infected with *Hymenolepis nana*. A similar infection was present in hamsters, an observation which has recently been reported by Watson[10].

The bearing of these results on the choice of a suitable laboratory host for experimental infections with *E. histolytica* in relation to the chemotherapy of amœbiasis is under investigation.

Our thanks are due to Mr. F. Welch of this Institute for the photomicrographs.

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SCIENCE IN SOCIAL AFFAIRS

ON November 22, a conference of social scientists, convened by the Association of Scientific Workers, was held at Gas Industry House, London. The chair was taken by Mrs. W. Raphael, of the National Institute of Psychology, and the principal speakers were Prof. V. Gordon Childe and Mr. Roy Innes, general secretary of the Association.

The primary aim of the meeting was to enlist support for a draft report on "The Furtherance of the Social Sciences" prepared by a committee of the Association and shortly to be published. The meeting was the third of its kind, the first having been held in October 1945 to explore the interrelations between natural and social science, at which a Joint Sciences' Committee had been set up, and the second in December 1946 to consider the future of the social sciences. Some months earlier, the Joint Sciences' Committee had been reconstituted as the National Committee on the Social Sciences of the Association. The conference was well attended by workers in the cognate social disciplines at universities, Government departments and research institutes.

In her opening remarks, the chairman observed that social scientists are becoming a distinct and self-conscious body of workers, which may be due partly to the unpopularity which they share, particularly among some sections of the Civil Service, but more because the demand for their services is growing; although well-qualified persons are in short supply. The conference, she said, had the practical aim of inviting support for certain resolutions included in the draft report. This document had been prepared against the background of the report of the Clapham Committee on "Provision for Social and Economic Research", which was considered by many competent to judge as altogether too timid in its recommendations. She thought the national situation was such as to demand a much more vigorous approach to the whole problem of placing the social sciences upon a sound footing.

Prof. Gordon Childe illustrated the development of the social sciences by briefly sketching the history of anthropology, drawing a parallel with the growth of natural science; in both cases theory and practice had been closely interwoven. In the eighteenth and nineteenth centuries, traders, travellers, missionaries and Government representatives had begun to have direct dealings with primitive peoples in Africa, North America, the Pacific and other far-flung areas. Generally speaking, in spite of good intentions they were seriously handicapped in the concepts at their disposal, which had been assimilated in their own society. A theoretical basis for the understanding of other cultures was lacking. For example, they tended to assume that ownership of land everywhere conformed to the canons of Roman law. Their methods were thus as unscientific as primitive efforts to control the wind by appealing to a 'wind god', and injustice and administrative difficulties were the result. Nevertheless, the accumulated descriptions of primitive customs were suitable as data for scientific study and led to the discovery of new facts.

Both in Great Britain and in the United States the theoretical background of early anthropologists had been borrowed from the idea of evolution in the form of Lamarckism. To Tylor, Taylor, Morgan and others, property under capitalism and Christian marriage seemed to be the culminating forms of these institutions. The new study of prehistoric archæology arose out of the rectification of the theory of unilateral evolution which had long been a stumbling-block. Prof. Childe remarked that the inadequacy of early anthropological theory no more means that social science is impossible than the exposure of the phlogiston theory disproved the existence of chemical science.

In commenting on the proposed report, he pressed the need for 'pure' research to proceed side by side with the application of its results, and with this end in view financial provision should be forthcoming, new academic chairs and other posts created in the right numbers, and ample opportunities for work in Government service. Early students of anthropology, like their colleagues in the natural sciences, were men of leisure and private means, a source of supply which has run dry. The first academic anthropologists were drawn from other disciplines or from the ranks of administrators or retired Civil servants. Although this first generation was somewhat sterile in matters of theory, it produced students trained to observe primitive life, who returned from their field experience equipped with new interpretations. Anthropology had advanced in response to social needs. Prof. Childe referred in passing to the Euclidean methodology of academic economics, a subject which, like anthropology, has been lately regenerated by the infusion of the results of realistic experience.

The Clapham Report he thought rather out of date in its outlook, particularly in the modesty of its claims, which was due to a pessimistic view of the availability of qualified personnel. Reluctance to expand on these grounds, while admitting the need for new posts, sets up a vicious circle. Nor did he agree with the Clapham Committee's view that social scientists do not need expensive apparatus for research; their demands, he believed, are comparable with those of natural scientists, though their 'laboratories' may be sited overseas.

Prof. J. D. Bernal, in opening the discussion, said that the draft report was admirable in the way it set out the role of the social sciences as experimental disciplines; but it needed strengthening and extending in certain particulars. The report dealt with the internal position in these subjects and not with their link with the natural sciences. Like Prof. Childe, he singled out economics for special comment. Economists, he suggested, regard materials and technique as constant factors, while taking prices, market trends and interest rates as variables, whereas natural scientists take the reverse position. In fact, both sets of factors are variables. Hence the need for multi-disciplined research teams, which could be most useful in relation to present economic difficulties. A more

immediate return could usually be expected from social than from natural science, inasmuch as the improvement of incentives, morale and organisation involved less delay and less dislocation than competition for capital goods. Experiments in social science, Prof. Bernal agreed, are costly, and he referred to the experimental houses now being studied by the Ministry of Works. The whole sociological investigation may cost a million pounds before it is finished. It may also happen, as sometimes occurs in the natural sciences, that a large-scale inquiry fails.

Prof. Bernal also agreed that it is urgently necessary to create senior posts in the social sciences, even if the available candidates have not attained eminence or maturity of years. In the early nineteenth century it was not usually decided to leave university chairs of science vacant rather than appoint persons who had not yet achieved fame. For example, Hamilton became professor of physics at Dublin at the age of twenty-two. If this practice were followed in the social sciences, the whole level of activity in the field might be raised. In any event, Prof. Bernal was doubtful whether so-called 'maturity of judgment' is an essential condition for success as a social scientist.

Prof. Bernal pointed to the value of regionalization in social science, by which he means the cultivation of local surveys in which people living in the area participate. Every area should have a team of social and natural scientists for this work, and out of this would come that co-operation between observer and observed which is so vital in social science. Indeed, this distinction might better be obliterated altogether.

Subsequent speakers expressed sympathy with the substance of the draft report and its recommendations, subject to certain amendments.

Prof. T. H. Marshall thought that the inadequacy of the financial need as assessed by the Clapham Committee is now fully recognized, and it would be a pity if the new report were committed to a definite figure. Grants should not be confined to universities; Government departments, research institutes and voluntary bodies concerned with social services or the development of techniques also need support. He felt that the position of sociology in the universities is extremely serious, facilities for study or research being virtually non-existent. The invariable difficulty of deciding on an occupant for a chair could be dealt with as suggested by Prof. Bernal.

Prof. Marshall argued in favour of permanent social research or survey units at universities, providing for some posts which do not carry teaching duties. The factor of size of the institution should also be examined. In a large institution, administrative and supervisory burdens are so great that they prevent the professor from getting on with his proper work. The solution is to be found in creating more senior posts and in delegating much of the teaching.

The plea for more senior posts in the universities was strongly supported by Dr. M. Fortes and several other speakers, without fear of suitable persons not being available. Dr. Fortes also compared standards. It seemed to him that at Oxford it is possible to win a higher degree in physical science by the patient manipulation of instruments without the investment of much thought; but in the social sciences the path is more difficult. The order of priority in the establishment of new university appointments in the various social sciences should, he felt, be carefully scrutinized and also the location of posts in areas where particular types of inquiry could most profitably be pursued.

The practice of the Royal Society in not admitting social scientists to membership received adverse comment. It is absurd, one speaker suggested, for ancient religious and political prejudices to determine limits of this kind; but speedy change of policy is unlikely, since the last reform of the Royal Society, initiated in 1833, has not yet been fully achieved.

A proposal in the draft report envisages an organisation of the same type as the Medical and Agricultural Research Councils, with advisory panels the chief function of which would be to stimulate research through the distribution of funds. Objections to this proposal were of two kinds: first, that a body might result interested in welfare to the exclusion of research, and second, in the absence of an independent body such as the Royal Society, social research would become dominated by Government control with all the possible ensuing dangers. Prof. Bernal believes that something of the kind is certainly needed if only to establish the intrinsic importance of the field, and he suggested that two bodies, one official and one independent, might serve the purpose. Another speaker favoured a council in the form of a 'federation' of social research units in Government departments and elsewhere. The most notable omission from the Clapham Report, he said, was the failure to recommend the setting up of social research units in Government departments like the Ministries of Labour, Health, and others primarily concerned with human organisation and relations. This would not preclude the use of mixed research teams when necessary. A social science research council, if formed, should be rooted in research units of this type as a condition of its success. It was eventually agreed to refer back for further clarification the whole section dealing with the proposal for a social science research council.

Mr. R. Innes, general secretary of the Association of Scientific Workers, said that the Association has always supported the idea of the unity of all branches of science in respect of their common interests and problems. It therefore welcomed the initiative displayed in preparing the report, and the executive committee had approved the recommendations in principle. The draft report had been circulated for comment to some 250 persons known to be active in the field. When the revised document was printed, the widest discussion would be needed wherever the possibility existed of implementing the recommendations. In these discussions natural scientists should take part, for they did not always fully appreciate the significance of the work of their colleagues. It was the hope of the Social Sciences' Committee that those present as well as others would help to implement the recommendations at universities and elsewhere. The Social Sciences' Committee would try to take a leading part in this. He added that a survey is in preparation of the fields of work, methods of training and career prospects in the social sciences.

Finally, the Conference adopted four resolutions: (i) urging publication of the report; (ii) supporting in general its recommendations; (iii) inviting the Association to support all steps leading to implementation; and (iv) requesting the Association to submit the report for the consideration of the Lord President of the Council, the Minister for Economic Affairs, the University Grants' Committee, the Vice-Chancellors' Committee and other individuals and organisations concerned.

JOHN COHEN

INTERNAL STRESSES IN METALS AND ALLOYS

ON October 15 and 16, a symposium on internal stresses in metals and alloys was held in the rooms of the Institution of Mechanical Engineers. It was organised by the Institute of Metals, in association with the Faraday Society, the Institute of Physics, the Institution of Mechanical Engineers, the Iron and Steel Institute, the Physical Society and the Royal Aeronautical Society, and was intended to bring together all classes of investigator interested in the subject, and to enable them to compare their points of view. In this it was quite unexpectedly successful, for the large lecture room of the Institution of Mechanical Engineers was filled throughout the programme, and discussion continued with a reasonably good balance between the different interests for the whole of the allotted time.

The proceedings were opened by Colonel P. G. Gueterbock, president of the Institute of Metals, and were divided into four sessions. The first dealt with the measurement of internal stresses. A considerable number of methods have been employed, but none is really convenient and reliable. The trend of the discussion suggested that for the measurement of the stresses which are broadly distributed through the mass of the metal (the *macro* or *body stresses*) the method of suitably cutting the article and measuring the resultant distortions is still the most generally applicable, though it has the disadvantage that it usually involves the destruction of the article. Some speakers thought that the technique of this method has not been adequately studied and developed. The X-ray method emerged as clearly the most suitable for the measurement of stresses which vary considerably within short distances (the *micro* or *structural stresses*); while those methods which depend on the measurement of properties such as electrical resistivity, magnetic properties and damping capacity proved to be open to criticism. The existence of variations of these properties can readily be demonstrated, but it is always difficult to decide in a particular case how much of the variation is due to internal stress and how much to other causes. Some time was devoted to the lattice strains detected by X-ray methods in plastically deformed metal. It is coming increasingly to be realized that, on account of its polycrystalline structure, plastically deformed metal is a mass of diverse stresses and strains; that this is also the case in plastically deformed silver chloride was elegantly demonstrated by Mr. J. F. Nye by the use of polarized light.

The afternoon of the first day was devoted to sixteen papers on the origin and control of internal stresses in metals. So many papers could scarcely be discussed fully in one afternoon, but the great variety of the conditions which can give rise to internal stresses in metals was brought out very vividly, and it was possible to pick out those questions which exercise the minds of the engineer and metallurgist at the present time. The possibility that internal stresses may cause distortions in components of high-speed machinery is a source of anxiety, for lack of balance can induce dangerous stresses; and it is more than likely that some special treatments designed to give strength or toughness result in higher internal stresses than should be tolerated. In some aluminium alloys the treatment which gives the freedom from internal stresses may result in liability to stress corrosion effects, and a nicely balanced treatment is necessary if the producer is to err on neither side. The last word has not yet been said on the influence of the stresses left behind by the welding process, and although there is no evidence that they are necessarily harmful, most engineers prefer to remove them if it is at all possible. On the other hand, the advantages to be obtained by deliberately introducing compressive stresses at the surface of components are regarded as demonstrated; but it is also felt that more, and more complete, data are needed on the effect of these stresses on the fatigue limit of the component.

The second morning was devoted to the effects upon metals of the stresses distributed on a microscopic scale, and produced a general discussion of the theory of the strength of metals. Sir Lawrence Bragg opened with an attractive development of his theory of the effect of crystallite size on strength, and emphasizing the necessity of thinking in terms of strain rather than of stress, directed attention to the great difference in the mobilities of simple and complex faults in crystal lattices. Several speakers referred to the uncertainty of present measurements of crystallite size, and the ultimate conclusion appeared to be that measurements of crystallite size, X-ray line broadening and mechanical properties are wanted, carried out on the same specimens, and under widely varying conditions. A recurrence of interest in the conditions at grain boundaries was noticeable, and the vexed question of the ability of dissolved hydrogen to burst solid steel, which is so important in connexion with the defects known as hair-line cracks, was also debated. In this argument, much depends on what is assumed about the molecular condition at room temperature of the dissolved hydrogen, on which subject we are still insufficiently well informed. Little disagreement is now expressed about the origin of the internal stresses associated with reactions in the solid state, such as precipitation hardening and the formation of martensite. The complementary question of the effect of stresses on the course of the transformations is not so clear, the distinction between the effects of stress and of plastic deformation not always having been kept in mind.

The final discussion, which dealt with the effects of stresses distributed on a macroscopic scale, was largely concerned with stress corrosion cracking, and revealed a considerable unanimity of opinion. There were differences in the estimates of the susceptibility of different materials to this type of failure, and of the importance of particular factors; but all speakers agreed that corrosive conditions favourable to the production of localized pits, together with sufficient tensile stress at the surface of the metal to cause plastic deformation at the roots of the pits, are the essential factors. The well-known cracking of certain stressed metals in the presence of molten tin or lead does not, however, fit very well into this picture. A second theme which ran through this discussion was the spontaneous cracking which sometimes occurs in steels, particularly hard steels, in the presence of high internal stresses. Mr. J. F. Russell showed some very impressive examples of such ruptures in large steel ingots and forgings, but in practice the phenomenon has occurred chiefly in welded hard steel plates, and has caused some anxiety. Corrosion appears not to be concerned in this case, and the reason why the cracks appear only after the lapse of time is not yet clear. The cracks can, however, be prevented by suitable choice of the

composition of the steel, and, more important, by ensuring that the steel is not put into use in an excessively hard condition.

In all, thirty-six papers were presented at the symposium and these, together with a report of the discussions, will be published in the Special Reports and Monographs series of the Institute of Metals. The symposium left a strong impression of the diversity and importance of the problems associated with internal stresses in metals, and the published account will prove to be a most valuable source of information.

GROUND BLOCKS OF THE CONTINENTS AND OCEAN BOTTOMS*

By Prof. H. CLOOS

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DURING the last twenty or thirty years, the scientific conceptions of the structure and development of the earth's crust have been widely influenced, if not dominated, by the Wegener theory of continental drift and by the idea of orogenetic phases dividing the structural history into short but world-wide times of movement separated by intervals of rest or of mere 'epeirogenetic' oscillations. The first theory disrupted the lateral, the second one the vertical (temporal) connexions. But within the same decades, the growth of geological knowledge of nearly all continents, combined with the results of submarine and even of geophysical investigations, pointed in another direction. They led to the conclusion that the major structural zones and units of the earth are much older than they formerly seemed, and that they indicate a long and strong continuity from the very first stages of the globe up to the present. It seems that from early geological time, the crust has been divided into polygonal fields or blocks of considerable thickness and solidity, and that this primary division formed and orientated the later movements.

My own contributions to this extensive problem date from 1910, when I first investigated the influence of old Tertiary faults upon young Tertiary folds within the Swiss Jura Mountains near Basle. Later on they dealt with the Rhinegraben, with the primary limits of the Rhenish Schiefergebirge, the Precambrian stages of important block boundaries in Norway and Sweden, with the conservative development of Silesia and the Bohemian mass, with the Harz Mountains, with North-East and South Africa, with certain areas in North America, and the Atlantic structure, the Azores and the Red Sea. Among the many important observations of other scientific investigators in different countries, Dixey's discovery of an older Nyasa fault-trough in Africa is the latest but not the least. Sonder, in his papers on Europe and Central America, laid stress upon the straight direction of certain old and persistent lineaments.

To start with the blocks, or fields, themselves, their limiting walls are to be considered as thick zones of a highly variable behaviour and history, and should be called 'geofractures' or 'geosutures', analogous to the similar terms 'geosynclines' and 'geanticlines'.

The critical features of these ancient and deep units and of their limiting zones are: (1) stratigraphical traces of tectonic movements in earlier geological times; (2) the connexion with plutonic and volcanic activity in early and recent times, etc.; (3) their relations to deep-seated earthquakes of linear arrangement (down to 500 or even 700 km.); (4) the length of the limiting zones (up to several thousand kilometres); (5) the size of the blocks, which varies between 500 and 1,200 or even 1,500 km. in diameter. My experiments and the known physical and technical observations show that there exists a geometrical relationship between the mutual distances of the dissecting joints and the thickness of the dissected sheet.

The crust pattern formed by these ancient geosutures displays several astonishing regularities. Within the three northern continents, blocks of north–south trend limited by meridional sutures are predominant. In North America, eight such fields can be counted; in Europe, eight north of the young mountain ranges in the south; in Asia, a few more. In the Mediterranean girdle, east–west zones prevail. On the Atlantic bottom, longitudinal zones of oblique directions are well known. Their analogy and connexion with terrestrial zones lead to the conclusion that they are the submarine representations of the ancient continental lines. It seems that many continental slopes belong to the deepest, oldest and most important block boundaries, an opinion already included in the well-known theory of the 'permanence' of the continents and oceans.

As areas of different horizontal and vertical displacement, the great thick permanent ground blocks influence the sedimentation on their upper surface, and also the magmatic cycles on their under surface within their interior and along their limiting walls. Being older than most of the other tectonic features of the globe, they influence the preparation, the origin and the development of the folded mountain ranges as well. Roughly speaking, within the two main thermodynamic girdles, each block has its own folded range. The structural relations between the younger folding and the older configuration of the respective blocks vary broadly according to the direction of the folding movement relative to the pre-shaped contour of the field. Longitudinal ranges, for example, are usually straight (Urals). Transverse ranges are suspended between the marginal sutures either as garlands or festoons, or in a sigmoidal manner or both (Füllhorn of the Alps). Thus attempts to unite the different folded arcs into uniform and endless 'slings' can be replaced by their individual adaptation to pre-existing units of the crust.

In certain examples, the deformation of a broad superficial area (like the Rocky Mountains in the United States or the corresponding area in Western Europe) can be derived from a more or less homogeneous deformation (for example, a simple torsional shear) of the supporting block. In a recent experiment I succeeded in imitating the structure of the Rocky Mountains. A sheet of liquid clay lies on a square wire net. Films of thin paper impregnated with water lie closely on the smoothed surface of the clay. A minute torsion of the wire net suffices to produce the Rocky Mountain uplifts with their typical details and their oblique arrangement. The mutual distances of the individual uplifts are pro-

* Summary of the presidential address given to the annual meeting of the Geologische Vereinigung at Bonn on April 29, 1947, under the title, "Die Grun ischollen der Festländer und Meere. Entwurf eines konservativen Erdbildes"; a detailed account is being published in the *Geologische Rundschau*, Stuttgart, No. 2 (1947).

portional to the number of sheets of paper applied, that is, to the thickness of the sheet of sediments. If the cover of paper is omitted and the other conditions are unaltered, the experiment produces the faults of the adjacent Basin and Ranges field. This variation corresponds exactly to the natural model.

Consequently, the tectonic deformations of the crust and the main features of its surface history can be more easily and better understood if we do not connect the visible structures directly with the supposed underground forces (subcrustal currents, convection as an effect of thermodynamic alterations, etc.). It is simpler to introduce an anisotropic and heterogeneous aggregate of pre-formed units which act as supports for the superficial material and as points of attack, as levers, and as instruments of transportation for the underground causal forces.

PRESERVATION OF THE *KAURI* IN NEW ZEALAND

IN a series of short newspaper articles in New Zealand, Prof. V. J. Chapman (professor of botany at Auckland University College) gives the conclusions he has reached on a problem of much interest to botanists and tree-lovers the world over, namely, the preservation of that remarkable tree, the *kauri* (*Agathis australis* Salisb.).

The only surviving extensive block of natural forest carrying big *kauri* is Waipoua Forest, covering some 30,000 acres and in the care of the State Forest Service. Here the *kauri*, including trees of outstanding size, occurs scattered throughout, mainly in groups. It is claimed that this tree may exceed even the Sequoias in timber content, owing largely to the remarkable way in which the trunk holds to the cylindrical form instead of tapering considerably towards the crown. Younger trees and saplings are to be found, but only in limited numbers. Experimental sowings or plantings have not always been successful, and though the N.Z. Forest Service is now more or less prepared to guarantee regeneration of this species, its critics do not yet appear convinced —a matter which it should be easy to settle on the spot.

Two main questions have arisen: What steps should be taken to ensure the protection of an adequate number of the existing big trees and the perpetuation of the species; and if for this purpose areas are excluded from green fellings, should dead and dying trees be utilized before they decay? It is alleged that the Forest Service has not hitherto publicly guaranteed complete protection from interference for any part of the forest, not even for the roadside strip, and that when felling for timber, usually takes all trees down to 30 in. in diameter and all dead and unhealthy trees fit for milling.

In Prof. Chapman's party, when he made the inspection of the forest on which his articles are based, the botanical and amenity interests were strongly represented, but unfortunately there does not appear to have been an experienced forester with them. To get the disinterested forestry point of view, he referred the matter as just another instance of a general problem to the sylviculturist of the Indian Forest Research Institute, and to the present reviewer, who has for many years campaigned among his colleagues and others for the preservation of outstanding trees and adequate samples of the forest types of which they form part: neither knew that Prof. Chapman intended publication. Meanwhile, the New Zealand Institute of Foresters has published a well-reasoned account of its view (*J. N.Z. Inst. Forest.*, 5, No. 3, 173–175; 1946).

There seems to be agreement that a portion or portions of the forest should be excluded from fellings with suitable safeguards against change of policy, as has already been done by the N.Z. Forest Service in a working plan already six years old. All living trees on a fairly wide strip on either side of the State highway should be, and indeed are, to be kept untouched in this way, for in New Zealand, as in Great Britain, the truth is that appreciative as a large section of the general public may be of woodland amenities and big trees, it rarely goes far from the road. A certain number of exceptional trees should obviously also be specially protected, and access provided: this also has already been decided on.

Scientific interests, including those of forestry itself, call for the preservation of a further compact block of forest (not a narrow strip), which would be excluded from ordinary management. One or two areas of about 250–500 acres each might be suggested, and there is a good case in favour of leaving in them even dead trees standing or fallen, as it is very likely indeed that some part of the fauna is dependent on the dead and decaying wood for its existence and survival. We understand one such area has already been set aside for the purpose.

Over the rest of the forest, normal management for timber production can be permitted. It is remarkable how fully the amenities of a forest area can be maintained with such management where it is the settled policy so to maintain them: the New Forest in England provides an example, thanks to the care of the deputy surveyor, for it is almost incredible that more than two dozen sawmills were working there throughout most of the war-years with so little consequent loss to the amenities or even change in appearance. Prof. Chapman urges that in the managed portion, *kauri* should be retained as the principal species, just as in Britain it has been decided in certain forests that broad-leaved trees should not be replaced by conifers: we believe this to be desirable and that the State Forest Service is willing to agree. But just how that will be done is not yet known, for on the information before us the *kauri* is a baffling tree, and a good deal more systematic research work will have to be done before it can be regenerated to order. Recorded information makes it clear that rather special conditions are required for the successful germination of the seed and establishment of the resultant plant; it is, however, not yet adequately known what those conditions are, though, as with many other trees, a primary requirement appears to be exposure of the mineral soil from its excessive covering of humus. The grouped occurrence, though by no means a unique feature in forest vegetation, has repeatedly been described, and studies are needed on soil and vegetational differences inside the groups and outside: the *kauri*, of course, itself influences the soil in which it grows and on which it showers its dead leaves and twigs. There is also a question of fungal association with the roots, which may incidentally play some part in the difficulties experienced in the nursery with this species. The importance of greater concentration on this research is underlined by the recommendation that until it is well under way only dead timber should be removed.

We feel sure that agreement will soon be reached in these matters, as there are no serious difficulties in meeting the requirements of both sides in the controversy. The forester of to-day has been trained to realize the amenity and biological aspects of the handling of forests equally with the timber-getting and economic aspects. The New Zealand Forest Service is fully competent to apply the agreed policy to this important forest and is the obvious authority to do so: its research staff is expanding, and though the problems presented by this exceptionally interesting tree are perhaps complex, they should be quite capable of solution in a few years. Meanwhile, it is as well that keen interest should be taken in the preservation for future generations of one of the most remarkable trees of the world. H. G. CHAMPION

ELECTRICAL PROPERTIES OF SOIL AND WATER AT CENTIMETRE WAVE-LENGTHS

THE electrical properties of the materials forming the earth's surface have always been of interest to the radio physicist and communication engineer, since the propagation of long and medium waves is markedly dependent upon the absorbing effect of the ground. At shorter wave-lengths, for which the radiation can be transmitted more freely through the lower or upper atmosphere, the reflexion coefficient of the earth's surface plays an important part in the propagation of the waves. More recently, the varying reflexion coefficient due to the different electrical properties of the different portions of the earth's surface has been used with conspicuous success in a radar method of navigation.

At moderate radio frequencies, the appropriate properties of soil and water can be determined by placing samples of the material in question in a condenser of suitable size and shape; and the results of many measurements made in this way at frequencies up to 100 Mc/s. have been described by R. L. Smith-Rose[1]. A laboratory method of measuring the electrical properties of salt-water solutions has also been used by R. Cooper[2] at frequencies up to more than 4,000 Mc./s., corresponding to wave-lengths down to 7 cm. Alternatively, the reflexion coefficient of the surface of the ground can be measured directly, for example, by the standing-wave method developed by J. S. McPetrie[3], who with J. A. Saxton[4] has used this technique at various frequencies up to about 600 Mc./s. (wave-length 50 cm.). At the still higher frequencies of 3,300 Mc./s., corresponding to a wave-length of 9 cm., L. H. Ford and R. Oliver[5] have investigated the direct reflexion of radio waves from a wide range of surfaces including bare ground, both level and in ridges, ground covered with vegetation, fresh water and salt water approximating in composition to sea water. More recently, this free radiation technique has been extended by J. A. Saxton and J. A. Lane[6] to the experimental determination of the dielectric properties of water for wave-lengths of 1·24 and 1·58 cm.

With the growth and development of radar techniques in recent years, the above-mentioned British work has been supplemented by the work of other investigators, notably in the United States, some of whom have paid particular attention to the measurement of the reflexion coefficient of sea water at the small grazing angles met with in radar practice.

A report, which has recently been submitted to *Nature*, describes measurements of the dielectric properties of soils and water at a wave-length of 3·2 cm. (frequency 9,400 Mc./s.) carried out at the University of Texas as part of the programme of the United States Office of Naval Research. The report is by Prof. A. W. Straiton and C. W. Tolbert, and is of interest in so far as particular attention was devoted to the measurement of the properties of the dry, sandy soil found in Arizona where the Navy Electronics Laboratory had a site for special radio experiments. The measurements were made by placing samples of the material in a wave-guide or other suitable circuit arrangement, and measuring the phase-difference and attenuation caused by transmission through different thicknesses of the material. From such observations the dielectric constant and conductivity of the dry Arizona soil were found to be 3·2 and 10^9 E.S.U. respectively. (A conductivity of 10^{10} E.S.U. corresponds to 1·1 Mhos/metre.) Similar measurements made on dry sandy loam taken from near the University Laboratory in Texas gave values for the dielectric constant of 2·8 and $0·7 \times 10^9$ E.S.U. for the conductivity. The authors of the report point out that the Arizona soil shows a conductivity of about three times as high as that obtained by Ford and Oliver[5] for very dry sandy loam in Great Britain, while the value for the Texas soil is much smaller. It is suggested that the considerable iron content of the Arizona soil may account for the higher conductivity in this case. This is scarcely consistent, however, with the results of further measurements made by the authors on samples of the Arizona soil sifted through screens of various sizes of mesh. The samples of smallest size of particle contained no free iron, but showed the highest conductivity. Furthermore, measurements made on the sample of medium-size particles varied from 6×10^8 E.S.U. when loosely packed into the container to more than 10×10^8 E.S.U. when very tightly packed. As was the case for measurements made by earlier investigators, the conductivity of the soils from both Arizona and Texas was found to increase several times when the samples were moistened with fresh water.

In a somewhat similar way, measurements were made under the same conditions on distilled water, and on a sample of sea water from the Gulf of Mexico. For the fresh water, at a temperature of 23° C., the dielectric constant was found to be 67 and the conductivity about 10^{11} E.S.U. (12 Mhos/metre); while for the sea water, which had a salt content of 2·2 per cent, the values obtained were 65 and 15×10^{10} E.S.U. (16 Mhos/metre) respectively for a temperature of 28° C. These values are in reasonably good agreement with the values deduced by Saxton[7] from a theoretical study of the anomalous dispersion of water combined with the results of measurements made at shorter wave-lengths than those used by Straiton and Tolbert. This recent work thus appears to form a useful extension of our knowledge of the electrical properties of soil and water at the frequencies corresponding to very short radio wave-lengths.

[1] Smith-Rose, R. L., *J. Inst. Elect. Eng.*, **75**, 221 (1934); *Proc. Phys. Soc.*, **47**, 923 (1935).
[2] Cooper, R., *J. Inst. Elect. Eng.*, **93**, III, 69 (1946).
[3] McPetrie, J. S., *Proc. Phys. Soc.*, **46**, 637 (1934).
[4] McPetrie, J. S., and Saxton, J. A., *J. Inst. Elect. Eng.*, **90**, III, 33 (1943); **92**, III, 256 (1945).
[5] Ford, L. H., and Oliver, R., *Proc. Phys. Soc.*, **58**, 265 (1946).
[6] Saxton, J. A., and Lane, J. A., Conf. Rept. Phys. Soc. and Roy. Met. Soc., 278 (1946).
[7] Saxton, J. A., Conf. Rept. Phys. Soc. and Roy. Met. Soc., 292 (1946).

FORTHCOMING EVENTS

*(Meetings marked with an asterisk * are open to the public)*

Monday, January 12

FARMERS' CLUB (at the Royal Empire Society, Craven Street, Strand, London, W.C.2), at 2.30 p.m.—Prof. D. B. Johnstone-Wallace: "Pasture Management and Animal Behaviour".

INSTITUTION OF ELECTRICAL ENGINEERS, LONDON STUDENTS' SECTION (at Savoy Place, Victoria Embankment, London, W.C.2), at 7 p.m.—Sir Arthur Fleming: "The Place of the Engineer in the Post-War World".

Tuesday, January 13

INSTITUTION OF CHEMICAL ENGINEERS (at the Geological Society, Burlington House, Piccadilly, London, W.1), at 5.30 p.m.—Mr. B. Edgington: "Detergency".

ILLUMINATING ENGINEERING SOCIETY (at the E.L.M.A. Lighting Service Bureau, 2 Savoy Hill, London, W.C.2), at 6 p.m.—Mr. C. B. Bicknell: "Applications of Modern Flash Discharge Tubes".

OIL AND COLOUR CHEMISTS' ASSOCIATION, LONDON SECTION (at the Royal Society of Tropical Medicine and Hygiene, Manson House, 26 Portland Place, London, W.1), at 6.30 p.m.—Colonel L. Urwick: "Scientific Management".

CONWAY DISCUSSION CIRCLE (at Conway Hall, Red Lion Square, London, W.C.1), at 7 p.m.—Mr. Lancelot L. Whyte: "Science and the Next Development in Man".*

SOCIETY OF INSTRUMENT TECHNOLOGY, NORTH-WEST SECTION (at the College of Technology, Manchester), at 7.15 p.m.—Mr. A. J. Young: "Some Impressions of Modern American Instrumentation".

Wednesday, January 14

SOCIETY OF CHEMICAL INDUSTRY, MICROBIOLOGICAL PANEL OF THE FOOD GROUP (joint meeting with the SOCIETY FOR APPLIED BACTERIOLOGY, at the Medical Society of London, 11 Chandos Street, London, W.1), at 2.15 p.m.—Mr. P. C. T. Jones: "Decomposition of Resins by Micro-organisms in the Soil"; Dr. D. M. Webley: "Microbiological Changes in Compost"; Mr. M. J. Geoghegan: "Influence of Micro-organisms on Aggregate Formation in Soil".

GEOLOGICAL SOCIETY OF LONDON (at Burlington House, Piccadilly, London, W.1), at 5 p.m.—Scientific Papers.

INSTITUTE OF PETROLEUM (at 26 Portland Place, London, W.1), at 5.30 p.m.—Mr. R. B. Richards: "The Structure and Properties of Ethylene Polymers".

INSTITUTION OF ELECTRICAL ENGINEERS, RADIO SECTION (at Savoy Place, Victoria Embankment, London, W.C.2), at 5.30 p.m.—Mr. D. M. Heller and Mr. L. C. Stenning: "Reference-Crystal-Controlled V.H.F. Equipments".

SOCIETY OF CHEMICAL INDUSTRY, PLASTICS GROUP (at the Chemical Society, Burlington House, Piccadilly, London, W.1), at 6.30 p.m.—Mr. E. G. K. Pritchett: "Recent Advances in the Chemistry of the Phenol-Aldehyde Condensation".

SOCIETY OF DYERS AND COLOURISTS, NORTHERN IRELAND SECTION (at the Queen's Hotel, Belfast), at 7.30 p.m.—Mr. G. Brearley and Mr. J. Starkie: "The Hydrosulphites—their Preparations, Properties and Textile Applications".

Thursday, January 15

INSTITUTION OF MINING ENGINEERS (at the Institution of Civil Engineers, Great George Street, London, S.W.1), at 11 a.m.—Fifty-fourth Annual General Meeting.

INSTITUTION OF ELECTRICAL ENGINEERS (at Savoy Place, Victoria Embankment, London, W.C.2), at 5.30 p.m.—Mr. D. E. Lambert and Mr. J. C. Christie: "Standardization of Switchgear".

ROYAL AERONAUTICAL SOCIETY (at the Institution of Civil Engineers, Great George Street, London, S.W.1), at 6 p.m.—Mr. O. N. Lawrence: "Gas Turbine Accessory Systems".

PHARMACEUTICAL SOCIETY (at 17 Bloomsbury Square, London, W.C.1), at 7.30 p.m.—Mr. A. G. Fishburn: "The Place of Pharmaceutical Research in Medicine".

ROYAL SOCIETY OF TROPICAL MEDICINE AND HYGIENE (at Manson House, 26 Portland Place, London, W.1), at 7.30 p.m.—Symposium on "Leprosy" (to be opened by Dr. Ernest Muir).

Friday, January 16

INSTITUTION OF MECHANICAL ENGINEERS (at Storey's Gate, St. James's Park, London, S.W.1), at 5.30 p.m.—Film on "The Screen in relation to Engineering Training" (introduced by Mr. H. R. Hockley).

INSTITUTE OF NAVIGATION (at the Royal United Services Institution, Whitehall, London, S.W.1), at 5.30 p.m.—Squadron Leader R. H. Blackmore: "Grid Navigation and its Allied Problems".

UNIVERSITY COLLEGE LONDON (at Gower Street, London, W.C.1), at 5.30 p.m.—Prof. C. A. Hart: "Modern Influences on the University Aspect of Professional Training in Surveying" (Inaugural Lecture).*

INSTITUTE OF PHYSICS, ELECTRONICS GROUP (joint meeting with the MANCHESTER AND DISTRICT BRANCH, in the New Physics Theatre, The University, Oxford Road, Manchester), at 7 p.m.—Dr. J. G. Wilson: "The Cloud Chamber".

PAPER MAKERS' ASSOCIATION (TECHNICAL SECTION), NORTHERN DIVISION (at the Engineers' Club, Albert Square, Manchester), at 7 p.m.—Mr. F. North: "Some Factors Affecting the Application of Dyestuffs to Paper".

Saturday, January 17

BRITISH PSYCHOLOGICAL SOCIETY (in the Physiology Theatre, University College London, Gower Street, London, W.C.1), at 2.30 p.m.—Mr. Alastair Heron: "Adolescent Reactions towards the Idea of Education for Parenthood".

APPOINTMENTS VACANT

APPLICATIONS are invited for the following appointments on or before the dates mentioned:

PRINCIPAL OF THE MINISTRY OF SUPPLY SCHOOL OF ELECTRONICS, Malvern—The Chief Superintendent, T.R.E., Great Malvern, Worcs., quoting Ref. 5/138/JFA (January 17).

ASSISTANT AGRICULTURAL ECONOMIST (Grade B)—The Registrar, The University, Bristol 8 (January 17).

LECTURERS (2) IN MATHEMATICS—The Clerk to the Governing Body, Battersea Polytechnic, Battersea, London, S.W.11 (January 24).

LECTURER IN CHILD HEALTH—The Registrar, The University, Manchester 13 (January 24).

SENIOR LECTURER in charge of the DEPARTMENT OF ELECTRICAL ENGINEERING under the Professor of Engineering—The Registrar, The University, Sheffield (January 31).

PRINCIPAL SCIENTIFIC OFFICER (physicist, preferably with good experience of military operational research), a SENIOR SCIENTIFIC OFFICER (experimental physicist, preferably with good experience of the scientific investigation of the performance of military weapons and equipment), a SENIOR SCIENTIFIC OFFICER (biologist or botanist or qualified in an associated science, with experience of military operational research and military training and of the scientific investigation of the performance of military equipment), a SENIOR SCIENTIFIC OFFICER (mathematician with experience of military operational research), and a SENIOR SCIENTIFIC OFFICER (mathematician with experience of the application of statistical methods to physical problems preferably in the field of weapon performance), at the Military Operational Research Unit, Byfleet—The Secretary, Civil Service Commission, Scientific Branch, 27 Grosvenor Square, London, W.1, quoting No. 2056 (January 31).

COW AND GATE FELLOWSHIP for research into problems associated with nutrition in infants and children—The Registrar, The University, Manchester 13 (January 31).

DIVISIONAL MANAGER OF (*a*) THE ANIMAL PRODUCTS DIVISION, (*b*) THE AGRICULTURAL PRODUCTS DIVISION, (*c*) THE MINERALS DIVISION, (*d*) THE CIVIL ENGINEERING AND WORKS DIVISION, (*e*) THE FACTORY DIVISION, and (*f*) THE FISHERIES DIVISION—The Director of Administration, Colonial Development Corporation, Dover House, Whitehall, London, S.W.1 (January 31).

INSTRUCTOR IN POULTRY HUSBANDRY AND POULTRY MANAGER at the College Farm—The Registrar, University College of Wales, Aberystwyth (February 1).

DIRECTOR OF THE MARINE LABORATORY, Aberdeen; PRINCIPAL SCIENTIFIC OFFICERS, SENIOR SCIENTIFIC OFFICERS, and SENIOR EXPERIMENTAL OFFICERS, in the Ministry of Agriculture and Fisheries and the Scottish Home Department—The Secretary, Civil Service Commission, Scientific Branch, 27 Grosvenor Square, London, W.1, quoting No. 2088 (February 10).

METEOROLOGISTS in the East African Meteorological Service—The Director of Recruitment (Colonial Service), Colonial Office, 15 Victoria Street, London, S.W.1 (February 21).

PROFESSORSHIP OF PATHOLOGY AND BACTERIOLOGY—The Secretary, Welsh National School of Medicine, 10 The Parade, Cardiff (February 21).

PROFESSOR OF PURE AND APPLIED MATHEMATICS in the Royal University of Malta—The Secretary, Universities Bureau of the British Empire, 8 Park Street, London, W.1 (February 28).

RESEARCH ASSISTANT for work on vitamins in relation to nutrition and allied microbiological investigations, and an EXPERIMENTAL OFFICER with experience in routine analysis of foodstuffs, blood and excreta—The Secretary, Rowett Research Institute, Bucksburn, Aberdeenshire (February 28).

ASSISTANT KEEPER to be responsible for the Zoological Collections in the Manchester Museum—The Registrar, The University, Manchester 13 (February 28).

LECTURER IN STATISTICS in the United College, St. Andrews—The Secretary, The University, St. Andrews (February 28).

ASSISTANT LECTURER IN GEOGRAPHY—The Registrar, The University, Manchester 13 (March 31).

SENIOR SCIENTIFIC OFFICER to the International Red Locust Control Service at Abercorn, Northern Rhodesia—The Director, International Red Locust Control Service, Abercorn, Northern Rhodesia (March 31).

LECTURER IN THE PHYSICS DEPARTMENT—The Secretary, Northampton Polytechnic, St. John Street, London, E.C.1.

AGRICULTURAL ZOOLOGIST in the Malayan Union—The Director of Recruitment (Colonial Service), Colonial Office, 15 Victoria Street, London, S.W.1.

LECTURER IN THE DEPARTMENT OF MATHEMATICS—The Principal, Borough Polytechnic, Borough Road, London, S.E.1.

LECTURER IN THE CHEMISTRY DEPARTMENT—The Secretary, King's College of Household and Social Science, Campden Hill Road, London, W.8.

LABORATORY ASSISTANTS—The Director, British Paper and Board Industry Research Association, St. Winifred's Laboratories, Welcomes Road, Kenley, Surrey.

AGRICULTURAL CHEMIST (Pyrethrum) by the Government of Kenya—The Crown Agents for the Colonies, 4 Millbank, London, S.W.1, quoting M.N.17954.

FISHERIES DEVELOPMENT OFFICER in Nigeria—The Director of Recruitment (Colonial Service), Colonial Office, 15 Victoria Street, London, S.W.1.

SENIOR ASSISTANT IN PHYSICS—The Director of Education, The Polytechnic, 309 Regent Street, London, W.1.

DIRECTOR OF THE CANTERBURY MUSEUM, Christchurch, New Zealand—The Secretary, Universities Bureau of the British Empire, 8 Park Street, London, W.1.

LABORATORY TECHNICIAN (Grade B)—The House Governor and Secretary, Gordon Hospital, Vauxhall Bridge Road, London, S.W.1.

TECHNICIAN IN THE DEPARTMENT OF PHYSICS—The Secretary, University College London, Gower Street, London, W.C.1, quoting Physics/1.